

**"OVER MY DEAD BODY":  
JAMES B. CONANT AND THE HYDROGEN BOMB**

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**Introduction**

The U.S. government's decision to develop the hydrogen bomb was a landmark of the nuclear arms race and a crucible of the science-military connection. Seeking a response to the unexpected and unwelcome news in the autumn of 1949 that the Soviet Union had exploded an atomic device, and with it the four-year American atomic monopoly, U.S. policymakers stood at a crossroads. One path was chosen on January 31, 1950, after four months of intense, sometimes bitter, and mostly secret debate within an elite stratum of government and military officials, scientists, and congressmen, when President Harry S. Truman, rejecting the advice of the Atomic Energy Commission's General Advisory Committee (GAC), endorsed a program to develop thermo-nuclear weapons. Despite its "minimalist" aspects (1), the outcome represented a clear victory for one faction of the policy elite and, at least potentially, a missed opportunity to restrain the nuclear arms race at a far lower level of destructiveness than in fact evolved. The decision, and the subsequent stripping of the security clearance of physicist J. Robert Oppenheimer, the GAC chairman who had argued against the H-bomb, also dramatized the ethical and political tensions present at the intersection of science and technology, military policy and strategy, and national and bureaucratic politics, and they offer a cogent case study of the technological and political forces driving the arms race (2).

In this paper I will focus on one of the key players in the drama of the hydrogen-bomb decision, a man whose career was intimately enmeshed with the birth of the nuclear age and whose part in the story of

the "Super" has often been underestimated or incompletely understood. From 1941, when he was the White House official assigned to monitor and assess the prospects for building an atomic bomb, through 1952, when his six-year term on the GAC ended, James Bryant Conant stood at or near the center of U.S. atomic policymaking. During this period, he was best known to the general public as the president of Harvard University, a post he held for two decades after he was plucked from the school's chemistry faculty in 1933. But Conant's atomic assignments proved his most fateful. In 1941 and 1942, his recommendations led to President Franklin D. Roosevelt's decision to approve an all-out effort to build the A-bomb. In May 1944 Conant was probably the first U.S. government official to devise a plan for international control of atomic weapons, and in the summer of 1945, as a member of the Interim Committee, he endorsed the decision to drop the bomb without warning on a Japanese city. After accompanying Secretary of State James F. Byrnes to the Council of Foreign Ministers talks in Moscow in December 1945, Conant helped formulate the Acheson-Lilienthal Plan, the first official U.S. proposal for international control of nuclear weapons. In July 1946 he turned down Truman's offer to head the newly created Atomic Energy Commission (AEC) — in part because of his distaste for Adm. Lewis Strauss, whom Truman had promised to name to the agency — but kept a hand in the commission's policies as a charter member of its prestigious General Advisory Committee. Composed of nine distinguished scientists who had played important parts in the Manhattan Project, and chaired by Oppenheimer, the brilliant theoretical physicist who had gained the title "father of the atomic bomb" for running the secret weapons lab at Los Alamos, the advisory panel met for the first time in January 1947; from its inception, it exerted a powerful influence on AEC policies (3). When the hydrogen-bomb controversy erupted, Conant was one of a handful of men in a position to consider the issues involved from the perspective of a decade of experience in atomic policymaking.

Before recounting Conant's role in the H-bomb controversy, I will examine the previous evolution of his views on thermonuclear weapons, his prior actions on the GAC, his perceptions of the cold war, and his relationship with Oppenheimer — all prerequisites for understanding

the stand he ultimately took. Like other allies in the struggle, such as Oppenheimer, David Lilienthal, and George Kennan, Conant opposed the hydrogen bomb in part because he adhered to a conception of U.S. national security policy different from that espoused by the Super's supporters; he shunned the view that nuclear weapons represented America's long-term strategic centerpiece, arguing instead the then-unpopular proposition that, in the long run, the United States would be less, rather than more, secure in a nuclear-armed world. In part, his opposition stemmed from a strongly held view that the weapon was a scientific and technological boondoggle — but it also reflected his personal philosophical and moral framework, his concerns about the changing relationship between science and the military, his fundamental judgments about human nature. On a personal level, Conant's stand in the battle over the Super signaled the fact that this was an issue, and a conflict, that touched the deepest chords of a man some associates considered cold and unemotional. Further, it climaxed his relationship with Oppenheimer, as the physicist and the university president ten years his senior bonded together in events that "recorded more than a political struggle; they seemed to involve the very destiny of man" (4).

When the battle was over, Conant and Oppenheimer found themselves on the losing side in a defeat that was to reverberate not only politically but in a deeply personal sense. Oppenheimer's fate, of course, is well known: the AEC revoked his security clearance in 1954, an action patently motivated by resentment of his antipathy to the hydrogen bomb. But, as this paper will demonstrate, the cleavage in the scientific community that exploded into the open at the Oppenheimer hearings had already played a key role in the plot to prevent Conant from becoming president of the National Academy of Sciences in 1950. Less visibly, but no less efficiently, Conant, like Oppenheimer, was targeted by scientists disturbed at his "lack of enthusiasm" for the hydrogen bomb and atomic energy, and who desired to discredit him as a shaper of atomic policy. He left the nuclear field partly as a result of his own disgust and despair, but, as will be seen, his exit from the pinnacle of the scientific establishment was hastened by a crisply executed power play, engineered by H-bomb advocates, that in some respects seems like a warm-up for the move against Oppenheimer. No



less than Oppenheimer's downfall, Conant's departure from the nuclear policy realm marked the triumph of "enthusiastic" scientific advisors intent on the unlimited development of nuclear weapons — what came to be known as the Strauss-Teller-Lawrence position — over those who placed higher priority on nuclear arms control.

### Conant and the H-Bomb, 1942–1949

The potential use of atomic fission weapons to set off thermonuclear reactions had darkened Conant's thoughts since 1942, when he first heard the idea discussed by scientists working on the atomic bomb. At that time, he was the White House's point man on the atomic project, holding the dual title of director of the National Defense Research Committee (NDRC) and deputy director of the Office of Scientific Research and Development (OSRD). As chairman of the "S-1 Executive Committee," he kept tabs on progress on the bomb for his boss and atomic-energy policy collaborator, Vannevar Bush; wrote frequent advisory memoranda, which Bush incorporated into his own prescriptions; and, for the duration of the war, occupied a unique position as intermediary between the Roosevelt White House and the scientists, industrialists, and military men working on the bomb (5).

When the question of man-made fusion initially arose, Conant had two principal concerns. First, he was aware that the theoretical possibility had emerged that an atomic fission explosion could ignite the Earth's atmosphere; thus causing a thermonuclear chain reaction that could wipe out life on the planet. Hasty calculations determined that the chances of this occurring were negligible — a "red herring," in the later words of Hans Bethe (6) — but Conant was dramatically reminded of this apocalyptic scenario when he witnessed the first atomic test in the New Mexico desert at Alamogordo on July 16, 1945. Momentarily blinded by the flash of the explosion, he believed for a moment that the burning of the atmosphere, the "end of the world," had actually begun. "Perhaps my impression was only premature on a time scale of years!" he wrote gloomily in his report to Bush the next day (7).

The second dimension of Conant's early view of hydrogen fusion concerned the possibility of constructing thermonuclear bombs capable

of producing yields of a far larger magnitude than fission weapons. Memoranda written by Conant during the war indicate that his estimate of the prospects for building a hydrogen bomb diminished somewhat between 1942 and the end of the war, but that its eventual reality consistently influenced his calculation of future developments in what he feared would be a secret atomic arms race between the United States and the Soviet Union. In September 1944, he and Bush speculated to Secretary of War Henry L. Stimson that a thermonuclear weapon (a "super-super bomb") might be built "within six months or a year after the first atomic bomb is constructed" (8). A few weeks later, however, Conant was more pessimistic after an inspection visit to Los Alamos at which he heard a report on thermonuclear developments from "the leading theoretical man" there — identified by Bethe (9) as none other than Edward Teller, whose evident fascination with fusion was already well known at the lab and a source of occasional friction with other scientists more interested in completing a fission device by war's end (10). "It seems that the possibility of inciting a thermo nuclear reaction involving heavy hydrogen is somewhat less now than appeared at first sight two years ago," Conant wrote Bush on October 20, 1944. Noting that the explosion produced by such a weapon could reach 100 million tons of TNT — compared to the eventual yield of 20 *thousand* tons of the Hiroshima bomb — Conant acknowledged that this "real super bomb is probably at least as distant now as was the fission bomb when you and I first heard of the enterprise" (i.e., about four years off, since Conant and Bush first learned of atomic bomb plans in 1941) (11).

Conant's new appraisal underlined the rough nature of the talk of hydrogen bombs at the time, and he was still seeking clarification on the issue when the Interim Committee convened in mid-1945; on May 31 — the same meeting at which the group endorsed use of the atomic bomb on Japan — the minutes note that Conant "mentioned a so-called 'third stage' of development [of atomic weapons] in which the products of the 'second stage' would be used simply as a detonator for heavy water" and asked Oppenheimer "for an estimate of the time factor in developing this phase." The physicist replied that this "far more difficult" stage would require a minimum of three years (12).



The uncertainty, even confusion, then clouding the prospects for development of a hydrogen bomb did nothing to recommend it to Conant, who prided himself on level-headedness and resistance to the "fancies" of physicists (13). (It was only after considerable skepticism that he was converted, in 1941, to the proposition that fission bombs could be built [14].)

But an even more important factor in Conant's lack of enthusiasm for the hydrogen bomb was his steady (with one apparent exception) conviction that fission weapons would prove quite adequate for American military purposes. "[T]he purpose of this memorandum," Conant had written Bush in October 1944,

is to show that even without [thermo nuclear weapons] the potentialities of a bomb of tremendously devastating effect are not far off if the efficiency of the fission bomb can be increased, as it undoubtedly can, by straightforward research and development along the lines now in progress (15).

The destructive power of fusion weapons further dramatized to Conant the transcendent moral and political dilemmas with which he was already grappling in relation to far smaller fission bombs. Well before the war ended, he was painfully conscious of the deadly path down which nuclear weapons threatened to lead humanity, and simultaneously, of the seeming impossibility of turning away from them. "We are to spend our money and our scientific manpower on a feverish race," he wrote Bush in May 1945, commenting on an appeal from Manhattan Project scientists in Chicago to maintain research at a wartime pace, "but when we have the ultimate what do we do with it? And if we can't answer that question, why enter the race?" Yet, while fearing a catastrophic conflict once the Soviets acquired their own bomb, Conant conceded that the United States must enter the race for nuclear supremacy even as it tried to halt it. "I am inclined," he wrote Bush, "to back an all out research program for the super-duper [presumably the hydrogen bomb] as first priority (leaving industry second role) and at the same time with equal priority push for an international armaments commission. We have about 5-10 years to do both!" (16).

On this one occasion, privately, Conant seemed to endorse building the H-bomb; but over the next four years his views reverted to a more

cautious stance, even though hopes to establish the international atomic energy control commission had dimmed by the GAC's first meeting in January 1947. At that session, Oppenheimer later recalled, the group actually "encouraged" research on the Super, and it made no move against such work prior to the October 1949 report (17). But from the outset, Conant, like other members, placed hydrogen-bomb work low on his list of priorities, for several reasons. First, research on the thermonuclear had failed to move very far beyond the stage it had reached during the war, and it still demanded the solution of critical theoretical obstacles before testing or construction could begin (and as Herbert York and Hans Bethe have noted, even these pessimistic forecasts proved unduly optimistic) (18). Second, with the production of fission devices grinding forward at a slow pace, the AEC concentrated on improving the efficiency of its program to stockpile Hiroshima-type bombs, especially given its limited financial, material, and manpower resources (19). Underlying these practical barriers was the lack, in the mind of Conant and of some other GAC members, of any obvious military or political need to jump from fission to fusion; as Oppenheimer was to say later, a bit eerily, even if it had been available during the war, the United States would not have used the hydrogen bomb on Hiroshima because "the target is too small" (20).

Conant's qualms about the H-bomb had hardened into firm opposition as early as March 1949 — not only after the shock of the Soviet atomic blast, as is usually assumed (21), and probably before Oppenheimer had made up his mind on the issue. Between February and October 1949, Conant chaired a secret ad hoc panel of prominent civilians under Defense Department aegis to review the issue of how much information the government should release to the public on atomic and other weapons of mass destruction (22). The staff man for the so-called "Fishing Party" was Lt. Col. Edwin F. Black. In early March 1949, Black dined alone with Conant at the Harvard presidential residence to receive details of his assignment; what he also heard was a fervent vow from Conant to quash the development of the hydrogen bomb. "He had a fixed opinion at the very beginning," Black, now a brigadier general, said in a 1985 interview; "Dr. Conant felt [the H-bomb] wasn't scientifically feasible and wanted to knock what he

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considered a crazy idea in the head once and for all." Taken aback by the intensity and rigidity of Conant's stand against the weapon, Black rapidly came to the conclusion that Conant was "determined to shape the group's report in that direction" (23). On the basis of a briefing by Pentagon experts, Conant helped prepare a committee working paper dated April 1949 on atomic weapons stating that "talk of superbombs of the order of magnitude of 50 or 100 times more powerful than the Nagasaki bomb are not based on fact" (24). And during the summer, as the group wrote up draft final reports, Conant urged the government to publicly declare that "atomic bombs have not been developed and are not now in the process of development which are in any way in the category of the super-bomb popularly discussed in the press" (25). (The committee backed this recommendation by a narrow 5-4 margin, but it seems doubtful that the final report, submitted on October 15, 1949, significantly influenced the H-bomb debate [26].)

### Conant and the Cold War

By the Soviet explosion, then, Conant was deeply skeptical of the scientific and technological prospects for the super; but his actions in the hydrogen-bomb debate cannot be understood without also understanding his perception of the political and diplomatic landscape. Conant's Cold War view fit comfortably in the mainstream of American opinion, but it differed significantly in several respects. While detesting communism, distrusting the Kremlin, and lacking faith in the short-term chances for negotiating an international control agreement with the Russians, he continued to believe that Moscow hoped to attain its aim of "furthering the world-wide spread of totalitarian socialism" primarily through political-ideological rather than military means. "We need to probe deeply into the fundamental question of what the Soviets' ambition would be if and when they have the atomic bomb in sufficient quantity and are ready with suitable methods of delivery," Conant told the National War College in a secret speech in September 1948. *my action was!*

I think it is extremely probable that the men who rule Russia do not dream of a military victory over the United States which would result in an occupation and control by Russian commissars, but rather a revolution in this country which would result in a totalitarian socialistic state with native American rulers (27).

Arguing against a preventive war on the "realistic" grounds that taking such an action would destroy the basis of American society and thus advance the very goal the Soviets seek, he urged policymakers to "walk a dangerous knife edge: on the one side is inadequate defensive and offensive power, antiquated ideas and equipment; on the other is domestic ruin, turning butter into guns." Peering into what he called the "age of the superblitz," when both Washington and Moscow possessed atomic arms in quantity, Conant managed to sound a hopeful note: *like?*

If the global war can be avoided and the Soviets find themselves confronted with a strong Western Europe, Great Britain and the United States, someday they may be willing to recognize a road block when they see it. Someday, when we are adequately rearmed and Europe's free societies have partially recovered from World War II, the Soviet rulers may be interested in a joint retreat from the age of the superblitz. If so, schemes for the international control of atomic energy will be once again on a realistic agenda (28).

That was the optimistic vision in Conant's crystal ball; the darker picture reflected his generally pessimistic view of human nature (29). Ever since becoming convinced of the feasibility of the atomic bomb, Conant had dreaded the war after World War II and, lacking the international control of atomic energy, the catastrophic destruction it would bring. In May 1944, the zenith of the Grand Alliance, he had privately written that the alternatives confronting humanity were a "race between nations and in the next war destruction of civilization, or a scheme to remove atomic energy from the field of conflict" (30). Characteristically, he planned for both outcomes, backing international control plans and secretly asking the Harvard University librarian to study the feasibility of microfilming and burying ten copies of "the written record of our civilization for the one that we can hope will follow" (31). As the postwar world polarized, Conant's analysis could take on a fatalistic, even chilling tone. "Perhaps the fated task of those of us alive now is to develop still further our civilization for the benefits of survivors of World War III in other lands," he wrote in 1948. "It would not be an inglorious mission" (32).

Apparently lacking a firm conviction as to which of the two anti-theoretical outcomes was more likely, Conant saw the Super as gratuitous to America's requirements for promoting a nuclear standoff, but potentially disastrous should a U.S.-Soviet war actually occur. For a time, in *yes*



the case of the atomic bomb, he had invested his faith in a technological advance to intimidate the Soviets and to ameliorate the dangers of an atomic arms race; these considerations, among others, had underpinned his support for the use of the atomic bomb on Japan in 1945, and led him to back the continued stockpiling and testing of U.S. nuclear weapons during the international control negotiations in the United Nations (33).

But in the case of the hydrogen bomb, Conant could not believe that building such a weapon would contribute to prospects for international control. By 1949, it had become clear that the American atomic monopoly had failed to intimidate the Soviets into agreeing to U.S. international control proposals, and the polarization of the wartime alliance into hostile armed blocs had rendered the issue all but moot. Recognizing these harsh realities, Conant and other consultants in March had urged the U.S. delegation to the U.N. talks to break off the discussions with the Russians; the Acheson-Lilienthal plan, which envisioned sharing atomic energy, and ultimately weapons, with the rest of the world, was "no longer applicable," he acknowledged. Truman "should make announcement that conditions have deteriorated," Conant urged. "We are playing with dynamite now since the Soviet might accept . . . When and if the Soviet is a friendly government we will have to make a new plan" (34).

When the news of the Soviet atomic blast arrived, it seemed to further cloud an already grim picture: a divided and militarizing Europe, with America's nuclear monopoly gone; an impending Communist triumph in China; and panicky fears of internal subversion. Worst of all, the "age of the superbomb," which Conant had forecasted for four years, was now clearly imminent. Against this backdrop, Conant that building felt the hydrogen bomb would only "louse up the world still more" (35). With increasing frequency, however, others began to latch onto precisely that idea in early October, and it was in this atmosphere that Conant, in the middle of the month, vented his feelings about the H-bomb in a letter to Oppenheimer shortly before the GAC was due to meet to consider possible responses to the Soviet advance.

### Conant and Oppenheimer

It was natural that Conant should express himself candidly to Oppenheimer, for the two had developed a strong personal bond during the Manhattan Project that deepened after the war. They held compatible political views, and saw each other regularly at GAC and other government advisory panels, and at occasional scientific or academic gatherings (36). Between them flowered a strong mutual admiration that sprung from their complementary strengths. Oppenheimer's brilliance and broadness as a physicist and thinker impressed Conant, who several times unsuccessfully tried to lure the scientist to the Harvard faculty. Oppenheimer, newly emerged from his academic cocoon, envied and sought to emulate Conant's savvy and experience as a Harvard and Washington administrator and operator. "Oppenheimer saw this faculty of Conant and wanted to learn from it," recalls physicist John H. Manley, a close associate of both men. "I could tell by the way he talked of Conant that he was very fond of him and relied on him a great deal" (37). Ten years Oppenheimer's senior, Conant had become not only his "mentor in national policy matters since 1942" (38) but someone he viewed as "a very wise, elderly person who in a normal sense of events he would like to have had as a godfather" (39).

Not one to suffer fools gladly, Oppenheimer was pleased during the Manhattan Project to find in Conant a government bureaucrat who could speak his own language, who could both grasp technical intricacies and empathize with scientists' philosophical concerns even when they did not conform to traditional military procedure. Along with Bush and Gen. Leslie R. Groves, Conant had approved Oppenheimer's appointment to direct the Los Alamos weapons lab, and several times cooperated with him to grease the government machinery to allay his worries about restriction of information, military discipline, and recruitment (40). As work on the bomb progressed, the two tall, thin men — the chain-smoking Oppenheimer almost wispy, Conant lanky, brisk, businesslike, and sufficiently youthful to be once mistaken for a graduate student (41) — conversed for many hours in the informal



atmosphere at Los Alamos during Conant's numerous inspection trips. By late 1944 their relationship had reached the point where Conant sent off letters to "Dear Oppie" and Oppenheimer affectionately addressed Conant as "Uncle Jim" (42).

Significantly, their closeness as administrators extended to substantive matters, as demonstrated near the close of the war when Oppenheimer, like Conant, supported the decision to drop the atomic bomb on Hiroshima (43). Oppenheimer's respect for Conant emerged publicly during the debate over atomic-energy legislation in the fall of 1945, when he broke with many fellow atomic scientists by endorsing the May-Johnson bill that Bush and Conant had helped draft. "I think no men in positions of responsibility, who were scientists, took more responsibility or were more courageous or better informed in the general sense than Dr. Bush and Dr. Conant," Oppenheimer told a congressional hearing. "I think if they liked the philosophy of this bill and urged this bill it is a very strong argument" (44).

Until their hopes withered under Cold War tensions, Conant and Oppenheimer energetically pushed for international control. In early 1946, both helped draft the Acheson-Lilienthal report outlining plans for an international atomic energy authority under U.N. aegis (45). If anything, as the talks foundered and Soviet-American relations worsened, Conant clung even more tenaciously than Oppenheimer to the belief that the Kremlin might yet come around to an acceptable position. Oppenheimer's stance during this period, Conant recalled in 1954, was "refreshingly, from my point of view, hard headed and anti-Soviet, which was my view at the time and always has been" (46). In fact, Conant admitted the possibility of a U.S.-Soviet atomic accord for at least a year longer than did Oppenheimer. By January 1947, the physicist was privately telling friends that he had given up all hope of reaching an agreement with the Russians (47). Nine months later, despite such ominous intervening events as the Truman Doctrine and Soviet refusal to join the Marshall Plan, Conant still urged military and government leaders not to rule out a "grand reversal" in Moscow's policies. "I cannot agree to write off the possibilities of an international agreement for the control of atomic energy however gloomy the prospects now appear," he told a National War College audience on October 2, 1947, floating the idea of forswearing nuclear power if that

would entice Moscow into serious bargaining (48). A dubious Oppenheimer wrote Conant, "I should be worried about embarking on a scheme whose collapse is probable and whose collapse would face us with the necessity for immediate military action" (49). Conant's hope still flickered in his reply:

Coupled with some generous offers on our part towards the Soviet Union, I really believe there is a chance for a grand international settlement of our outstanding troubles with that nation along these lines . . . I am afraid you'll think I have gone Utopian if I continue to write much more in this vein, so I will stop merely with the assurance that I trust I have not ascended in the stratosphere too far even in my thinking! (50).

The gap was still evident the following February when both men were consulted by the U.S. delegate to the U.N. talks, Frederick Osborn. "Conant indicated his belief that if the Russians would extend their proposals to permit a full and real inspection and drop the proposal for prohibition [of atomic weapons] first, then their plan might be better than no plan at all," Osborn wrote in his diary. Oppenheimer, however, felt

that the substitute of the Russian plan — however strengthened and improved — for the majority plan would be impossible, dangerous and unacceptable in the present state of the world. . . . If an ideal state of the world were attained — unlikely in our generation — then he might go along with Conant. Otherwise, [Oppenheimer considers] Conant's position absolutely untenable (51).

More important than this relatively minor disparity in their views — and by the fall of 1948 Conant, too, had conceded the talks' clear failure (52) — was the fact that Conant and Oppenheimer had established a strong personal tie that was "definitely" the closest on the GAC, according to Manley, the group's secretary. "They were kindred spirits, very sharp and perceptive, and that drew them together," Manley recalls. And, he adds, that relationship grew "most intense during the H-bomb period. That cemented them together" (53).

#### **Conant's Weariness with the Nuclear Program: A "Burned Out" Case?**

The collapse of international control efforts probably ended any doubts



Conant may have had about turning down the AEC chairmanship, a job whose attractiveness, he wrote in his memoirs,

rested on the assumption that negotiations then in progress in the UN Commission would result in positive action . . . . If I had thought of the position . . . in terms of being head of a vast arsenal for improving and manufacturing weapons (which is what the job actually became), I doubt if I would have spent long in declining the position (54).

Even in an advisory capacity, Conant was soon seized by mounting exasperation with the AEC's performance. The frustration erupted into the open, with ruffled feelings on all sides, at a turbulent June 3, 1948, GAC meeting at which the panel recommended a major reorganization. "We certainly have a wealth of advisers and watchdogs," a depressed ("I feel pretty *low*, frankly") and irritated AEC chairman, David E. Lilienthal, wrote in his journal (55). Conant had helped write the GAC's "undeniably sharp" (56) statement, which cited a lack of imagination and foresight on the commission's part, urged a large-scale decentralization of operations and authority, and concluded tartly: "We are afraid we can be of little use to the Commission under the present organization. We despair of progress in the reactor program and see further difficulty even in the area of weapons and production unless a reorganization takes place" (57).

Conant's pessimism extended beyond weapons to civilian nuclear power, which he scorned as "to a large extent only a mirage" (58). "The alleged benefits of atomic energy do not seem to me worth the price," he told a secret government audience in late 1948. "Atomic fuels can be too readily turned into atomic bombs to be safe for the civilized world of the twentieth century to handle. A self-denying ordinance is needed" (59).

Believing nuclear power to be largely a fraud, his once-rosy hopes for international control dashed, and exasperated by organization squabbles, Conant increasingly found his nuclear association a burden. He wrote Oppenheimer in March 1949 to suggest the GAC take a more active investigatory role — for "from the point of view of the general public until such time as individually and collectively we are ready to resign as a protest because of what we find, we are by our silence giving our blanket endorsement of the work of the Commission"

— and admitted that "in recent days I have taken what may have seemed a rather defeatist attitude about the work of the Committee" (60).

Conant's disillusionment surfaced that summer during a car ride from Berkeley to San Francisco with Luis W. Alvarez and Ernest O. Lawrence, both prominent physicists at Berkeley and avid H-bomb advocates. "Dr. Lawrence was trying to get a reaction from Dr. Conant on the possibility of radiological warfare and Dr. Conant said he wasn't interested," Alvarez recalled.

He didn't want to be bothered with it. I have the strong recollection that Dr. Conant said something to the effect that he was getting too old and too tired to be an adviser on affairs of this sort. He said, "I did my job during the war" and intimated that he was burned out, and he could not get any enthusiasm for new projects. So when Dr. Conant disapproved of the hydrogen bomb, I interpreted it in light of that conversation (61).

Historian Robert Jungk considers it "staggering" that Alvarez regarded Conant's behavior as evidence of being "burned out" rather than an expression of deep moral principles (62). But Conant was fed up, that much is clear — and in the hydrogen-bomb debate, he would have an opportunity to show that principles, not fatigue, motivated his actions.

### Fall 1949: Conant and the H-Bomb Debate

Conant's performance in the H-bomb drama, and in particular his still-missing letter to Oppenheimer vowing the weapon would be built "over my dead body," appears in retrospect as a kind of *cri de coeur* of his nuclear career, albeit characteristically restrained to official channels and shielded from public view. And while Oppenheimer is often depicted as the central figure in opposition to the hydrogen bomb, the evidence suggests that, rather than being swayed by Oppenheimer, Conant may well have emboldened Oppenheimer to make a stand in the controversy.

The issue of the so-called "Super" seeped through top government, military, and scientific circles following Truman's September 23 announcement of the Soviet test, although Conant was aware of the news even earlier, probably from Oppenheimer or Bush, both of whom had



helped evaluate evidence of the explosion (63). It is not clear when Conant and Oppenheimer first discussed their reaction to the Soviet bomb, but an opportunity arose on October 9 when Conant hosted Oppenheimer and three other new members of Harvard's Board of Overseers (64). They clearly conversed on GAC matters between October 11, 1949, when Lilienthal wrote Oppenheimer requesting that he call a GAC meeting to determine whether the AEC could do more for the "common defense and security" in light of the Soviet explosion, and October 14, when Oppenheimer replied and set the date for the weekend of October 29–30, "the first day on which both President Conant, who is quite busy," and Enrico Fermi could attend (65). Oppenheimer may already have received Conant's letter by then, for Kenneth S. Pitzer, then director of the AEC's research division, remembers that when he visited Oppenheimer at his Princeton home for a small Friday night dinner party, Oppenheimer took him aside into a study and either showed or described to him a letter containing Conant's views on the H-bomb and suggesting that he was taking a "similar" position; he believes the encounter took place on October 14 (66).

A week later, on October 21, Oppenheimer was visited in his office at the Institute for Advanced Study at Princeton by Edward Teller and Hans Bethe. In concert with Alvarez, Lawrence, and other Berkeley scientists, Teller had traveled across the country from California to lobby for an all-out effort to build the Super, his pet project for nearly a decade. Unlike Oppenheimer and Conant, these scientists had few qualms about exploiting to the fullest the destructive potential of the atom; they believed that arms control was a chimera, and that American security therefore depended on maximum military might, and specifically on aerial striking power to counter Soviet manpower advantages; and finally, they shared a romance with the intrinsic technical and scientific challenge posed by the thermonuclear, a lure to which not even Oppenheimer was fully immune (67). As part of his recruitment drive, Teller had called on Bethe, one of the pioneers of fusion research, in Cornell, but Bethe was torn over the prospect of returning full-time to weapons work. The two decided to visit Oppenheimer to hear his views on the subject. As evidenced by his letter dated the same day, October 21 (68), Oppenheimer had grave doubts about the Super — its feasibility, its morality, its military efficacy — but

on this occasion he played his cards close to the vest and did not express a strong position on the weapon, either pro or con. Instead, as with Pitzer, he showed or read his visitors a letter from Conant "which he said he had just received"; Teller told the Oppenheimer hearings that "one phrase of Conant's sticks in my mind, and that phrase was 'over my dead body,' referring to a decision to go ahead with a crash program on the thermonuclear bomb" (69).

Bethe, in his testimony, corroborated Teller's account. Oppenheimer had been "equally undecided and equally troubled in his mind about what should be done," Bethe said, and had disclosed that

one of the members of the General Advisory Committee, namely Dr. Conant, was opposed to the development of the hydrogen bomb, and he mentioned some of the reasons Dr. Conant had given. As far as I remember, he also showed me a letter he had written to Dr. Conant. As far as I remember, neither in this letter or in his conversation with us did he take any stand" (70).

In a January 1985 interview, Bethe clarified that he was shown a "very strong" letter written by *Conant* rather than Oppenheimer, and that "the gist of it was just like the sentence Teller quoted." He added: "The letter showed me that Conant and Oppenheimer were in very close contact" (71).

Had Oppenheimer displayed Conant's letter while cloaking his own view because he was still unsure of what stance he would take when the GAC met? Or, instead, did he prefer to preserve an air of impartiality as the panel's chairman while nevertheless hoping to sway opinion away from a crash program? Bethe, who in the end rejected Teller's overture to work on the hydrogen bomb at Los Alamos and publicly criticized Truman's decision (though he changed his mind and worked on the Super after the Korean War broke out in June 1950), believes the latter interpretation more likely: "Probably Oppenheimer wanted to influence us against the development of the hydrogen bomb and didn't want to do it in his own words, so he used Conant's letter instead" (72).

The incident raises the question of whether Conant and Oppenheimer were collaborating prior to the GAC meeting to oppose the H-bomb; if so, it was certainly not a very extensive effort, compared to the fanning out by pro-Super scientists to see politicians and military leaders during October. It also, at the same time, could be interpreted as lending further support, in conjunction with Conant's previously



described positions on the "Fishing Committee," to the view that Conant's opposition to the H-bomb exceeded Oppenheimer's in intensity. "Felt Oppie was lukewarm to our project and Conant was definitely opposed," was Teller's impression after the meeting, as recorded in Alvarez's diary (73). In any case, the accounts of Teller, Bethe, and Pitzer prove (and Oppenheimer's lawyers should have pointed this out) that Conant had forcefully transmitted his opposition to the Super *before* Oppenheimer's October 21 letter to him, thus both refuting insinuations by the AEC's attorney, Roger Robb, that Oppenheimer was attempting to "propagandize" Conant prior to the GAC meeting, and supporting Oppenheimer's recollection that Conant "told me what his views were before mine were clearly formulated" (74).

By the time Oppenheimer did formulate his views, on the morning of Bethe's and Teller's visit, Conant needed no convincing. Nevertheless, Oppenheimer's cogent, cautious letter may well have made an impression on Conant, for its reasoning echoed his own concerns over the excessive role he believed atomic weapons were coming to play in U.S. military planning:

What concerns me [Oppenheimer wrote Conant] is really not the technical problem. I am not sure the miserable thing will work, nor that it can be gotten to a target except by ox cart. It seems likely to me even further to worsen the unbalance of our present war plans. What does worry me is that this thing appears to have caught the imagination, both of the congressional and of the military people, as the answer to the problem posed by the Russian advance. It would be folly to oppose the exploration of this weapon. We have always known it had to be done; and it does have to be done, though it appears to be singularly proof against any form of experimental approach. But that we become committed to it as the way to save the country and the peace appears to me full of dangers.

The Super, Oppenheimer wrote, remained a "weapon of unknown design, cost, deliverability and military value," as it had been in 1942; but the human, rather than the technical, conditions had changed: two "experienced promoters," Teller and Lawrence, were at work selling the Super, and the Joint Congressional Committee on Atomic Energy (JCAE), "having tried to find something tangible to chew on since September 23rd [when the Soviet explosion was announced]; has at last found its answer. We must have a super, and we must have it fast" (75).

The most powerful ally of Conant and Oppenheimer in preferring an expanded fission program to the hydrogen bomb was Lilienthal, who

was aghast at the enthusiasm of the pro-Super scientists. "Ernest Lawrence and Luis Alvarez in here drooling over [Supers]," Lilienthal wrote in his journal on October 10; earlier he had conferred with Truman, still anticipating final approval for a reinvigorated, "whopping big" AEC weapons program *sans* Super (76).

The GAC itself was split as it assembled in the AEC's concrete headquarters on Constitution Avenue on the final weekend of October 1949 to discuss the Super. Conant missed the Friday session, when the panel heard a briefing from State Department Soviet expert George F. Kennan (also opposed to the Super) and conferred informally with Bethe. Discussion of the Super began in earnest on Saturday morning, with Conant present, along with seven of the GAC's eight other members: Oppenheimer, the chairman; Oliver Buckley, of Bell Laboratories; Lee A. DuBridge, president of the California Institute of Technology; Enrico Fermi, the Italian-born physicist who had directed the construction of the first atomic pile at the University of Chicago in 1942; I. I. Rabi of Columbia University, like Fermi a Nobel laureate in physics; Hartley Rowe, who had worked with Conant at NDRC and was now director of United Fruit Co.; and Cyril S. Smith, a metallurgist at Los Alamos. Only Glenn Seaborg, the Berkeley chemist, was absent.

Much has been written of the GAC discussion that ensued during the marathon sessions of Saturday and Sunday and the report that resulted, and there is no point in rehashing the material covered by *Atomic Shield* and *The Advisors*. What is important for an analysis of Conant's role is to clarify his actions, motivations, and influence. The most lucid record of Conant's views of the hydrogen bomb is his testimony before the Oppenheimer hearings, when he said that he opposed the weapon "as strongly as anybody on a combination of political and strategic and highly technical considerations":

Some of us felt then, and I felt more strongly as time went on, that the real answer [to the Soviet atomic bomb] was to do a job and revamp our whole defense establishment, put in something like Universal Military Service, get Europe strong on the ground, so that Churchill's view about the atomic bomb [that it prevented the Russians from reaching the Channel] would not be cancelled out.

One of the considerations was that [the hydrogen bomb] was sort of a Maginot Line psychology being pushed on us. On the technical ground the question was the investment in preparing certain materials which I am not going into, which are restricted,



which seemed at the time necessary; the use of materials which I don't want to mention, which would be used up.

The question was when you expended a certain amount of manpower and energy and material, would you actually from the point of view of delivering blows against a potential enemy be very much better off even if this line worked? (77).

The best contemporary record of the discussion, however (the GAC notes are unilluminating), suggests that another consideration, far less fashionable in the atmosphere of October 1949 (or April 1954), may have played a determining part in placing Conant strongly against the Super. Lilienthal wrote in his diary that Saturday night that Conant, "looking almost translucent, so grey," came out "flatly against it [the H-bomb] 'on moral grounds'" (the last three words were omitted in the published version but appear in Lilienthal's original journal entry). Oliver Buckley argued that there was no moral difference between scales of weapons, between "x and y times x"; but Conant, as one who had helped build and recommended the use of the atomic bomb, was sensitive on precisely that point: "Conant disagreed — there are grades of morality." Receiving support from Rowe and at least tacit backing from Oppenheimer — who as chairman did not express his own view until the others had spoken — Conant again stressed morality when Strauss, who as an AEC commissioner sat in for part of the discussion, commented that the final decision would not be made by a popular vote but "in Washington"; Conant replied, according to Lilienthal: "But whether it will stick depends on how the country views the moral issue" (78).

Further evidence of Conant's emphasis on the moral argument, as well as of his decisive role in prodding Oppenheimer to oppose the Super, appears in an unpublished 1957 interview of Oppenheimer conducted by political scientist Warner R. Schilling. Schilling wondered why Oppenheimer had shifted his position between October 21, when he had written Conant that it would be "folly to oppose [the Super's] exploration," and the conclusion of the GAC meeting nine days later, when he joined the rest of the group in forthrightly opposing the weapon's development. Oppenheimer explained, according to notes of the not-for-attribution interview, that the change "was a result of Conant's intervention. Conant said he just wouldn't have this, and pointed out that a firm stand could be expected to meet with the approval of various groups, churches" (78a).

For the often-stolid Conant — described by long-time friend George Kistiakowsky as an "unemotional, cold Yankee"; by another chemistry colleague, E. Bright Wilson, as a man with a "cold logical approach" to important decisions; and by fellow GAC member I. I. Rabi as "brisk," "business-like," "not much of a sentimentalist," "a red-tape man" — taking a stand on the hydrogen bomb seemed to involve an unusual degree of passion (79). "For him, taking part in this battle was very significant," said Rabi (80).

In some respects, Conant had come full circle since his work on the atomic bomb. At first dubious, by war's end he had believed that a few nuclear explosions and the threat of more could work diplomatic wonders; now that hope was gone. Formerly a proponent of the draft, in 1945 and 1946 he had doubted the need for universal military training "ever since I realized atomic bombs were in the offing" (81), and he considered the Air Force "the only branch of service looking to the future realistically" (82). Now he lamented the fixation on big bombs for strategic striking power and loudly advocated a rehabilitation of U.S. conventional capabilities and reinstitution of the draft. Having seen one weapon inspire such great expectations, he appears to have been a bit appalled by the thought of another gadget conjuring comparable visions: "This whole discussion makes me feel I was seeing the same film, and a punk one, for the second time," he muttered disgustedly at one point in the GAC discussions (83).

Why did Conant feel the moral issue was so significant? William L. Borden, who was to accuse Oppenheimer of disloyalty in part because of his opposition to the H-bomb, conjectured privately to the AEC and FBI in 1954 that Conant "never quite recovered from his World War I experience of working on poison gas; that he, Conant, had an emotional reaction to developing a horror weapon, and that has colored Conant's viewpoint ever since" (84). Borden's explanation, however, is unconvincing for several reasons. For one, there is no evidence whatsoever to indicate that Conant had any qualms about his work on poison gas, which in fact was not completed in time for use in the war (85). Secondly, that interpretation fails to account for Conant's central role in weapons development during World War II. And perhaps most significantly, it is important to recall that neither Conant nor the other members of the GAC were pacifists: the panel's report opposing the

nuclear!

(*Conant*  
*anxious?*  
*no*)



Super also urged an "intensification of efforts to make atomic weapons available for tactical purposes, and [for the AEC] to give attention to the problems of integration of bomb and carrier design" (86). As Herbert York points out, Conant, Oppenheimer, and other critics of the H-bomb, while finding nuclear weapons "repugnant," nevertheless "explicitly recognized the need to possess nuclear weapons, especially for tactical and defensive purposes, and they regularly promoted programs designed to increase their variety, flexibility, efficiency, and numbers" (87).

Like other GAC members who had worked on the atomic bomb, Conant differentiated the hydrogen bomb on the grounds that it "might become a weapon of genocide," in the words of the majority annex to the GAC report written by Conant and DuBridge and cosigned by Oppenheimer, Rowe, Smith, and Buckley. Because the Super was too big for use on military targets alone, the GAC said in its main report, signed by all eight attending members, "its use therefore carries much further than the atomic bomb itself the policy of exterminating civilian populations." The majority annex stressed: "Let it be clearly realized that this is a super weapon; it is in a totally different category from an atomic bomb" (88).

But one must delve further to understand Conant's rationale. He had not, after all, shied away from the wholesale destruction of cities during World War II, helping to produce not only the atomic bomb but other weapons used in strategic bombing. "If the American people are to be deeply penitent for the use of the atomic bomb," he had written theologian Reinhold Niebuhr in 1946,

why should they not be equally penitent for the destruction of Tokyo in the thousand plane raid using the M69 incendiary which occurred a few months earlier? ... I was as deeply involved in the one method of destruction as the other, so at least on these two points, I can look at the matter impartially (89).

There is nothing in the record to support Barton Bernstein's claim that guilt over Hiroshima and Nagasaki contributed to Conant's stand on the hydrogen bomb (90); his only regret, publicly and privately, was that the atomic bomb wasn't completed and used earlier (91).

Though not explicitly mentioned, a crucial distinction between the decisions to build the atomic and hydrogen bombs was the context

surrounding them. One of Conant's guiding dictums, and one he firmly believed to justify his wartime work, was that during war one has no choice but to employ whatever means are needed or available to win. "Let us freely admit," he had said in 1943, "that the battlefield is no place to question the doctrine that the end justifies the means." After the war, however, must come a restoration of morality: "But let us insist, and insist with all our power, that this same doctrine must be repudiated . . . in times of peace" (92). This same theme permeated an article published by Conant in the January 1949 *Atlantic*, in which he affirmed his "deep-seated conviction" that "war is always totally different morally from peace" and suggested that "acceptance of the doctrine that the end justifies the means would be the moral equivalent of dropping atomic bombs on a dozen of our own cities" (93). Even more than the atomic bomb, the hydrogen bomb posed the problem of the means threatening to overwhelm any conceivable end to which they could be employed; but whereas Conant had been able to rationalize the dropping of the atomic bomb on Hiroshima not only on military grounds but with the hope that the action would actually promote postwar international control efforts, he had no illusions that this would be the case with the Super (94).

Conant had worked for the completion of the atomic bomb with a war in progress and with the fear, until almost the close of the conflict, that the Germans were racing forward on their own bomb. In fact, Conant had been startled (if relieved) to find out just how little progress the Germans had made — perhaps because the discovery implied that it had been unnecessary for the United States to build the bomb in the first place. Echoes of that experience were sounding in Conant's mind as he opposed the H-bomb; despite the explosion of "Joe One," he was more skeptical of Soviet than of American chances for producing a fusion bomb, and he was quite skeptical of the American chances. Could 1949 represent a chance not to repeat the fateful turn taken in 1939?

The language of the majority annex, written by Conant, implies a belief that the Soviets would not have completed an atomic device by August 1949 without the successful example of the American atomic bomb; and that therefore it might prove possible to forestall the advent of the new weapon simply by not proving that it could work:

\* a major Secret Warning (which was strong, authoritative, present (right), timely — and stopped secret — and was over-revel. (Clifford)

\* Did Conant change mind after Teller-Alan discussion? (later Office) If he had known this earlier...?



We believe [the majority annex concluded] a super bomb should never be produced. Mankind would be far better off not to have a demonstration of the feasibility of such a weapon until the present climate of world opinion changes.

It is by no means certain that the weapon can be developed at all and by no means certain that the Russians will produce one within a decade. To the argument that the Russians may succeed in developing this weapon, we should reply that our undertaking it will not prove a deterrent to them. Should they use the weapon against us, reprisals by our large stock of atomic bombs would be comparably effective to the use of a super.

In determining not to proceed to develop the super bomb, we see a unique opportunity of providing by example some limitations on the totality of war and thus of limiting the fear and arousing the hopes of mankind.

The main body of the report reflected the priorities of Conant, Oppenheimer, and the rest of the GAC, as well as Lilienthal: accelerated production of fissionable materials, increased emphasis on tactical atomic weapons, and the production of freely absorbable neutrons for experimental and military purposes. As for the feasibility of the Super, the panel predicted that "an imaginative and concerted attack on the problem has a better than even chance of producing the weapon within five years." While noting that there was no theoretical boundary to the yield of super bombs, the GAC nevertheless stated flatly that "there appears to be no chance of there being an economical alternative to the fission weapons" on a dollar-per-damage basis (95).

The full GAC also adopted Conant's "firm" proposal that the government declassify enough information about the Super to enable public debate and to state clearly where the government stood on the issue (96). Ideally, the GAC hoped, Truman would issue a statement forswearing development of the Super, explaining its destructive potential, and specifying that no nonmilitary benefits would be sacrificed by not going ahead (97).

From Lilienthal's account, it appears that Conant had an important part in swinging the GAC to a position of unanimous (minus Seaborg) opposition to the Super at its October meeting. Lilienthal writes that during the Saturday discussion as many as five of the eight members present ("less than half of the 8, never more than 5") seemed to favor an all-out development program for the H-bomb, but on Sunday all agreed to sign the group's report, and only two Fermi and Rabi — signed a minority annex making their renunciation of thermonuclear

weapons conditional on Soviet agreement (98). DuBridge, in an interview, said Conant's arguments were "forceful, but so were some of the others" (99); Rabi recalls that "Oppenheimer followed Conant's lead" in the discussion (100). On Monday, October 31, Lilienthal telephoned Conant to congratulate him on the outcome of the meeting and told him the decision might have gone the other way had he not stood his ground (101).

The GAC report, however, also galvanized the supporters of the Super as the battle lines in the debate began to emerge. Advocates of the weapon longed for a return to the sensation of superiority enjoyed during the years of monopoly, feared the Soviets might beat America to the weapon, and, conversely, eyed the gains available to Washington if it won the race. To Lewis Strauss, one of five AEC commissioners, the hydrogen bomb represented a "quantum jump" in atomic weapons and "the way to stay ahead" of the Russians (102), while the Joint Chiefs of Staff confidently predicted that U.S. possession of the Super would "grossly alter the psychological balance between the United States and the USSR," at least until the Kremlin developed its own H-bomb (103). Such sentiments were shared by two other significant forces rallying behind the weapon: the cavalcade of pro-H-bomb scientists who reached Washington in October to seek out sympathetic ears in Congress, the government, and the military, and Senator Brien McMahon, the powerful chairman of the Joint Committee on Atomic Energy.

By the time the GAC met again on December 3 — reaffirming its earlier stand, with Seaborg now present and Conant ardently reiterating his previous arguments (104) — the secret debate was raging at full steam, and Truman had appointed a special three-man committee to make a final recommendation: Lilienthal, Secretary of State Dean Acheson, and Secretary of Defense Louis Johnson. Lilienthal strongly supported the GAC's position, now coming under increasing fire, and Johnson just as clearly was determined to develop the weapon (105). That left Acheson, an early proponent of international control but now an inveterate Cold Warrior, as the potential swing man, and Lilienthal asked him to see some opponents of the Super before making up his mind.

One of these opponents was Conant, whom Acheson hosted for lunch on Wednesday, January 18, 1950 (106). By then Acheson had all

(Fall of China?)



but decided to back development — he had been unable to fathom how the GAC believed the United States could “persuade a paranoid adversary to disarm ‘by example’” (107) — but from the one surviving document that refers to their luncheon, it seems Conant came tantalizingly close to swaying Acheson into the *contra*—H-bomb camp. The next day Acheson wrote that he had decided to recommend going ahead with the Super, but “after listening to Conant it would be very easy to arrive at the opposite conclusion, except that in arguing against the position I had come to, he admittedly could not suggest an alternative” (108).

Truman also saw no alternative to going ahead. The option of resurrecting the ghost of international control in the form of a mutual pledge not to build or test an H-bomb he considered neither feasible nor desirable. Building up military power seemed the most prudent method of dealing with the Soviets — while unilaterally forgoing, or offering to negotiate the fate of, the Super, it was feared, would be perceived as a sign of weakness. Even if, as the GAC believed, there was no clear military requirement for the weapon, its chief value, as Gen. Omar Bradley had said, was “psychological.” And with anti-Communist fears and passions rising — it was the fortnight of Alger Hiss’s conviction and atomic spy Klaus Fuch’s confession, and the midst of the debate over “who lost China?” — even appearing to concede any advantage to the Soviets was politically unacceptable. When the Acheson-Johnson-Lilienthal committee met with the president on January 31, they were told that “there has been so much talk in the Congress and everywhere and people are so excited he really hasn’t any alternative but to go ahead and that was what he was going to do” (109). Truman announced that he was directing the AEC to “continue” — Lilienthal’s euphemism — “its work on all forms of atomic weapons, including the so-called hydrogen or super-bomb” (110).

### “Like a Funeral Party”

Truman’s decision to go forward on the hydrogen bomb sharply curtailed the Oppenheimer-led GAC’s influence. Though they continued to meet regularly to offer technical advice, there was a wide-

\* Hb the last decision difficulty — would the Acheson-Johnson-Lilienthal committee not have occurred — when it did?

spread perception that the sharp rebuff of the panel on so important an issue had irretrievably damaged the group’s standing within the government.

Gathered for its scheduled January 31 meeting, the GAC was plunged into gloom when Lilienthal relayed Truman’s decision. Not only would research on the Super go ahead, but Truman had rejected Conant’s “firm” view that the matter should be publicly debated and had slapped a secrecy order on the advisors. “It was like a funeral party — especially when I said we were all gagged,” Lilienthal wrote in his journal. “Should they resign? I said definitely not, on the contrary. This would be very bad. Though before long a number of them may, just because they feel their standing is impaired” (111).

Ironically, in view of the accusations later hurled at the GAC for allegedly delaying the H-bomb program even after it had become official policy, Conant later wrote privately that he and Oppenheimer “didn’t [resign] (or at least I didn’t) because I did not want to do anything that seemed to indicate we were not good soldiers and did not do what we could to carry out orders of the President!” (112).

In retrospect, Conant felt, the two should have departed immediately after the decision (113). Instead, he gloomily watched near-panic gripping the capital as, within a week of Truman’s H-bomb announcement, a British scientist working on the Manhattan Project, Klaus Fuchs, was revealed to have been a Soviet spy, and Senator Joseph McCarthy waved a sheet of paper he claimed contained the names of 201 (or 57, or 81) Communist traitors working in the State Department. “I hope you are standing up under the strain of these trying times as well as usual,” Conant wrote Oppenheimer on February 14, enclosing a copy of a letter he had received from a friendly Washington reporter disclosing that a Republican senator on the JCAE was spreading the story that the GAC had opposed the H-bomb program “on moral grounds” (114). One can sense Conant’s exasperation from a cryptic passage in a letter to Bernard Baruch ten days later:

When I am in Washington, it seems as though I were in a lunatic asylum, but I am never sure who is the attendant and who the inmate. Nor am I even sure whether I am a visitor or a potential patient. However, I am trying to keep my sanity and will do what I can with the others (115).

on CANDOR

why?

after!



On March 2, 1950, Conant had a chance to insert his views directly into the policy-making machinery, when he was called in for consultation on a major internal review of Soviet-American relations in light of the Soviet atomic explosion and the U.S. decision to seek the hydrogen bomb. The document that later emerged from the review, NSC-68, was a seminal statement of the American view of the Cold War, urging a major U.S. military buildup to forestall the danger of Soviet superiority within four to five years, defining in stark terms Moscow's "design for world domination," disdaining immediate prospects for negotiation, and issuing a clarion call for the United States to "bring about an internal change in the Soviet system" and liberate those under Kremlin domination (116).

Disturbed by the sweeping definition of U.S. objectives, Conant engaged in a sharp debate with NSC-68's architect, Paul H. Nitze, the State Department's new director of policy planning. The goal of rolling back and eventually eliminating Soviet rule could not be achieved short of war, Conant suggested, and had Nitze "considered the fact that in World War III, we might, in winning the war, lose our freedom"? As before, Conant's sights were much more narrowly construed; rather than the "Utopian objective" of uprooting Communist rule, he felt that "for the next 20 years our objective should be to live on tolerable terms with the Soviet Union and its satellites while avoiding a war." Nitze responded, foreshadowing the rhetoric of John Foster Dulles's "roll-back" schemes, that "if we had objectives only for the purpose of repelling invasion and not to create a better world, the will to fight would be lessened" (117).

Disagreement also flared over the advisability of arms talks. While NSC-68 would express grave doubts about the wisdom of seeking controls of atomic weapons, Conant refused to concede the defeat, or irrelevance, of the view that it was in America's long-term security interest to shift away from them, and he asserted that "the atomic bomb is a bad weapon from the United States point of view." Despite Truman's decision, and the grim state of U.S.-Soviet relations, Conant still pushed for negotiations with the Soviets, perhaps to reign in atomic-energy development. To Nitze's claim that a failure in talks could raise tensions to a flash point, Conant argued pragmatically that

even the failure to reach a settlement "would be a very strong argument for the necessary sacrifices on the part of the United States" and "might put the Soviet Union in a hole in the Cold War."

What America needed to do, Conant felt, was to build up Europe economically, put a million troops on the ground there to defend it, and not waste its resources on developing new weapons such as the hydrogen bomb. Disdaining visions of moving to the "offensive" in the cold war, Conant stressed the longer-term goal of avoiding global war for the next few decades. (By 1980, he predicted, the Soviets' "absurdities and static system would cause them to grind to a stop. He repeated that if we can hold what we have, especially the United Kingdom, and avoid war, then the competition between our dynamic free society and their static slave society should be all in our favor, or if not, we deserve to lose[!]" (Recent reforms in China and the Soviet Union suggest that this was a rather prescient prediction.)

#### Another Defeat: The "Incident" at the Academy

Though down, the GAC was not yet out of the picture — a fact that continued to rile those scientists who were irked by the panel's opposition to the H-bomb and its alleged lack of faith in nuclear power. As the newly anointed thermonuclear program mounted a recruitment drive, the tensions that had sprung up during the previous fall lingered, and some of that acrimony emerged when Conant was nominated for a post that figured to crown his position as leader of the American scientific community: the presidency of the National Academy of Sciences (NAS).

The uprising that deprived Conant of the presidency of the academy in an unprecedented revolt from the floor at a closed session in April 1950 has been attributed to a number of factors, the most innocuous of which was concern over the fact that Conant — who had first made his mark as a prize-winning organic chemist and had won membership in the NAS in 1929 at the unusually young age of thirty-six (118) — would inevitably be a part-time leader, since the bulk of his time would still be spent at Harvard. But almost immediately dark rumors began to flourish that other motives were decisive. For seventeen years the



incident remained a family secret of the scientific community, until the appearance of an article in *Science* magazine in 1967 attributing Conant's defeat to "vengeance . . . exacted in a vendetta seething since World War II" (119).

Seeking a replacement for academy president A. N. Richards, who had announced plans to retire, a nominating committee in early 1950 selected Conant, perhaps the most illustrious figure in American science but not the most active figure in NAS affairs. In the past, such selections had been routinely rubber-stamped by the full membership. The post carried immense prestige, a \$15,000 annual salary, and both ceremonial and substantive responsibilities (120).

As planned, Conant's name was placed into nomination at the April 24, 1950, meeting (which Conant did not attend). But, the original minutes of the meeting read, "From the floor, Mr. W. M. Latimer nominated Detlev W. Bronk as candidate for President. Mr. V. K. LaMer seconded the nomination" (121).

Bronk, the president of Johns Hopkins University and chairman of the academy's National Research Council, had urged Conant to stand for president, and he disclaimed any foreknowledge of the uprising from the floor (122). Both Wendell Mitchell Latimer, dean of chemistry at the University of California at Berkeley, and Victor K. LaMer, a Columbia University chemist, had worked under Conant in the NDRC during the war, and Latimer in particular was known to be unhappy at Conant's performance. It was quickly evident that a "revolt of the chemists" was under way — including a number of chemists who for more than five years had been nursing resentment over what they considered Conant's wartime misadministration (123). "They ganged up on him behind his back," commented Conant's close friend, chemist George Kistiakowsky, still angry more than three decades later (124).

Confusion erupted on the floor of the convention, during which Bronk requested that his name be withdrawn; instead, another chemist rose to make a "very effective . . . 'drafting'" speech in his favor (125). A vote was then taken among the 201 members in attendance, "with the result that a majority of votes was cast for Mr. Bronk" (126). When Conant was reached by telephone at the Baltimore home of Harvard corporation member William L. Marbury and told that opposition had

arisen, he withdrew from contention and endorsed Bronk (127). Bronk was academy president; Conant was humiliated before his fellow scientists.

Though Conant refused to speak of it publicly, and claimed in his memoirs that as the years went by he grew "happier and happier with the outcome and soon almost forgot the incident" (128), the rebellion by his fellow chemists left a bitter aftertaste. Kistiakowsky recalls that Conant was "terribly upset by this dirty deal that was engineered by the chemists" (129), and "furious about what he regarded as a revenge by a few West Coast chemists feeling slighted in W. W. II and a real doublecross by Detlev Bronk" (130). After Conant's death in 1978, Kistiakowsky wrote his widow:

Probably the most painful incident of Jim's life as science leader occurred without warning to me and without my being able to take any steps to prevent it, an event which I see as a tragedy to American science as well as a disappointment I know to Jim. I refer to Jim's withdrawal from nomination as the next president of the National Academy of Sciences, when suddenly confronted by a small but secretly well organized group of little men who resented Jim's wartime leadership. The rest of us were unaware of what was being organized and thus were unable to demonstrate to Jim in good time the strong support which in fact would have been his. Jim's sensitive personality, of course, led him to withdraw rather than wage a political battle, which for his opponent was largely that for status and position. Had Jim become the president of the Academy, I know he would have raised its influence in Washington and made it into the center of science policy leadership in America and abroad, an objective the realization of which largely escaped . . . the men who led the Academy since those days (131).

Conant made no such charge in his memoirs, but he believed, according to Kistiakowsky, that Bronk had "urged him to stand for election but then conspired with the malcontents to be nominated 'from the floor' during the Academy meeting without warning Jim . . . until post factum" (132). In fairness to Bronk — the "innocent beneficiary" of the anti-Conant revolt (133) — it must be stressed that no evidence has emerged to support this contention. One scientist reported privately several months later that Conant's opponents "picked Bronk more or less whimsically," and the NAS history notes that Bronk was elected "over his protests as a friend of Conant" (134). But doubts sprang up almost immediately. One scientist wrote a month after the event of "ugly rumours going around Harvard and Hopkins and presumably



elsewhere that *Bronk* himself conspired & assented in advance to the *coup*," but added that "if he did he is a more perfect dissembler than anybody I ever saw in action . . . he asked that the floor nomination of his name be withdrawn and when the Academy went ahead and elected him he was so completely taken aback that he lost all his aplomb for just once in his life and was determined to decline the election," had Conant not endorsed him and withdrawn his own candidacy (135).

The outcome had immense consequences for the National Academy of Sciences and for Bronk and Conant personally. Bronk headed the institution until 1962 — a span that included the Korean War, McCarthyism, Sputnik, the Oppenheimer case, the Cold War, and a vastly expanded role for the academy and for science in government affairs; how it would have fared under Conant is a matter of speculation.

What is clear, however, is that the event marked a turning point in Conant's life. He later wrote that had he taken that job, he doubted he would have accepted Eisenhower's invitation in 1953 to go Germany, where he served for four years as U.S. high commissioner and ambassador, or embarked on his subsequent career as self-appointed ombudsman of public education (136). While for a short period he continued to serve on government science panels, he made a sharp, if private, break with the scientific community and, according to one report, "never again set foot in the Academy building" (137).

Unraveling the murky tangle of motivations that led to the academy's revolt against Conant is a difficult task. "Undoubtedly the spearhead of the opposition was personal, but I heard later that cogent arguments were used," Conant wrote in his memoirs (138). "No one is in a position to assess the motives of the individuals who voted to elect Bronk," declared one of the leaders of the upheaval, Berkeley chemistry professor Joel H. Hildebrand, in a letter to *Science* vehemently rebutting the idea that a "vendetta" against Conant was responsible (139). The contention that Conant would only be a part-time president; genuine support for Bronk; and the residue of resentment toward Conant, still festering among certain Manhattan Project veterans, all had a role.

There is, however, a chain of evidence, some of it circumstantial,

some of it direct, linking the carefully planned and executed maneuver not only to these causes but to controversy over the hydrogen bomb. The nucleus of the opposition — centered at, but not confined to, the Radiation Laboratory at the University of California at Berkeley — consisted of scientists whose prior dissatisfaction with Conant was now reinforced by strong antipathy to his position on the Super and his close ties to Oppenheimer. This previously overlooked connection suggests that the successful power play against Conant was a harbinger of the cleavage of the scientific community that was to explode into the open in the Oppenheimer case four years later, and reflected the growing ascendancy of the faction of scientists who ardently favored the hydrogen bomb, nuclear power, and atomic weaponry as the mainstay of U.S. national security policy.

The prime agitator against Conant was Latimer, dean of chemistry at Berkeley, delicately described by Conant as "a California chemist to whose ideas I had not always lent a sympathetic ear" (140). According to Kenneth S. Pitzer, a colleague in the Berkeley chemistry department, Latimer resented Conant's hiring of "second rate Harvard people" to fill chemistry posts in the war effort when "more technically expert people were available whom Conant didn't happen to know personally. . . . This caused a certain amount of resentment . . . such things never completely disappear" (141). In Alvarez's view, Latimer was "a little paranoid about Conant" (142).

Latimer's testimony before the Oppenheimer inquest reveals an almost pathological loathing for both Oppenheimer, whom he saw at the center of a diabolical conspiracy of scientists, and Conant, whom he saw as one of Oppenheimer's henchmen (143). In the fall of 1949 Latimer had catalyzed the convoy of pro-H-bomb scientists from Berkeley — Teller, Alvarez, and Lawrence — to descend on Washington prior to the October meeting of the GAC, a group he regarded as being completely under Oppenheimer's spell. "You know," Latimer told the hearing,

he is one of the most amazing men that the country has produced in his ability to influence people. It is just astounding the influence he has upon a group. It is an amazing thing. His domination of the GAC was so complete that he always carried the



majority with him, and I don't think any views came out of that Committee that weren't essentially his views (144).

Specifically including Conant as "under the influence of Dr. Oppenheimer," Latimer averred that Conant's technical reasons for opposing the hydrogen bomb "sounded pretty phony to me . . . I doubt it was a free judgment on his part" (145). (Conant testified that he relied on Fermi rather than Oppenheimer for nuclear expertise [146].)

Latimer left conclusive evidence to confirm the link between the H-bomb and NAS affairs when he was secretly interviewed by an FBI agent and AEC attorney Roger Robb, as they prepared for the Oppenheimer hearings:

Latimer stated he had helped defeat Dr. Conant in the election for President of the National Academy of Sciences and elect Dr. Detlev W. Bronk since he knew that Oppenheimer "had Conant in his hip pocket" and was promoting Conant's candidacy (147).

Latimer was not the only H-bomb proponent who in the spring of 1950 recognized and rued the influence of Oppenheimer and Conant on American nuclear policy: Pitzer himself appears to have played a major role. According to Lawrence R. Hafstad, a close associate of Pitzer's at the AEC, Pitzer was the leader of the revolt (148). "I do not know what Pitzer felt about the explosion," one scientist wrote Bronk a week after the Academy meeting. "He took no public part in it; but he did not appear downcast after it" (149). A Berkeley chemist who had become director of research at the AEC in early 1949, Pitzer strongly favored an immediate push for the Super as the necessary reaction to the Soviet explosion. According to FBI documents, he had been "amazed" when Oppenheimer did not share his enthusiasm and "surprised to learn from Oppenheimer that he (Oppenheimer) had talked with Conant who professed the same views. [Pitzer] said he did not know, but suspected Oppenheimer had persuaded Conant" (150).

Near the end of Conant's advisory service, Pitzer charged that unnecessary delays in the AEC's programs had occurred; he said that some GAC members — Conant in particular — had shown "remarkably little enthusiasm" for the commission's goals and should be replaced by

advisors "with faith and enthusiasm for the job to be done" (151). Pitzer's hostility to the anti-H-bomb view ran so deep that, by May 1952, he privately told the FBI that he was "now doubtful" of Oppenheimer's loyalty because of his opposition to the weapon (152). In a 1985 interview, Pitzer confirmed that he "was of the Latimer viewpoint at the Academy" and, while citing other reasons to oppose Conant's nomination, added: "You can say that Conant's position on the hydrogen bomb . . . may well have been a significant motivation for a minority of the voters" — including him and Latimer (153).

Less clear are the activities of several colleagues of Latimer and Pitzer who, as strong H-bomb advocates actively engaged in the development of the weapon, may well have looked askance at any bolstering of Conant's prestige. Lawrence and Alvarez, who had been encouraged in their H-bomb lobbying by Latimer, had been disappointed by Conant's lukewarm response to new possibilities for radiological warfare, and they believed that Oppenheimer had consistently demonstrated bad judgment in atomic policy matters and had dominated the GAC. According to FBI documents, Alvarez felt that "Oppenheimer and his group had put the University of California on the black list" (154). Pitzer believes that Lawrence was not "an active conspirator" in the NAS affair, while Alvarez recalled talk of the action but denied participating (155).

One Berkeley physicist whose view of the academy action seems clear even if his role, if any, remains a mystery, is Edward Teller — the driving force behind the H-bomb. By April 1950 Teller saw Conant as an enemy in the struggle for control of U.S. atomic policy, an impediment to the H-bomb program (he had seen Conant's "dead-body" letter), and an Oppenheimer ally. Conant, Teller told the FBI in 1952, "is outspoken in his opposition to the H-bomb and even to the atomic bomb [and] against further work in atomic energy for peace-time use as he is of the opinion that better results can be accomplished through the use of solar energy" (156). A month before the NAS meeting, Teller conveyed his concerns about Conant to the executive director of the Joint Committee on Atomic Energy, William L. Borden: "I feel the attitude of the members of the GAC has been a serious difficulty in our recruiting efforts. . . . A man like Conant or Oppenheimer can do a



great deal in an informal manner which will hurt or further our efforts" (157). Borden's reply expressed doubt that either Conant or Oppenheimer would prove helpful to the program (158). (Three years later, Borden would trigger the hearings into Oppenheimer by writing FBI Director J. Edgar Hoover that the physicist was "more probably than not" a Soviet spy.) In March 1954, shortly before the hearings, Teller made his view of the academy incident clear to an AEC agent:

Teller talked . . . about the "Oppie machine" running through many names, some of which he listed as "Oppie men" and others as not being on his team but under his influence. He says the effort to make Conant head of the National Academy of Sciences is typical of the operation of the "Oppie machine." He adds that there is no organized faction among the scientists opposing the "Oppie men" (159).

"Undoubtedly involved" in the move against Conant (according to Pitzer), including "corridor conversation" on the day of the revolt, was Harold C. Urey, a Nobel laureate in chemistry from Columbia University and a leading scientist-activist (160). A member of the S-1 Executive Committee chaired by Conant, Urey had sharply criticized the administrator for alleged bureaucratic bungling, and Conant believed he had complained behind his back to other dissatisfied project scientist, an action that struck him as "extremely disloyal" (161). At war's end Urey had battled on the opposite side of the controversy over atomic-energy legislation. Having disagreed with Conant over the decision to use the atomic bomb, Urey in 1950 also opposed him on the hydrogen bomb, vocally promoting the "Strauss-Lawrence" line before Truman's final decision and, Lilienthal wrote in his diary, spreading "innuendo on [the] GAC" (162).

Another pro-H-bomb NAS member, Willard F. Libby, a chemistry professor at the University of Chicago, shared the perspectives of the anti-Conant cabal, though no direct link has been established. During the Manhattan Project, Libby had been a group leader in the section working on isotope separation at Columbia University — a source of discontent with Conant's administration; Urey had been dissatisfied in particular with Washington's handling of the research Libby conducted (163). In the fall of 1949, both Urey and Libby had been contacted by Latimer shortly after the Soviet atomic explosion, and both responded

favorably to his pro-H-bomb pleas (164). Libby, who joined the GAC in August 1950, also shared Latimer's view that Conant (and Lee DuBridge) usually backed Oppenheimer on atomic matters and "perhaps . . . were taken in or persuaded in their views" by him (165).

### End of the Road: Conant Leaves a "Bad Business"

Conant's setback at the National Academy of Sciences, veiled from public knowledge, by no means signaled his withdrawal from national affairs: After the Korean War erupted in June 1950, he helped found the Committee on the Present Danger to promote public support for a massive military build-up, for the stationing of U.S. troops in Europe, and for a full-scale draft — in essence, the program he had pushed to Nitze. And as a member of the Scientific Advisory Committee to the Office of Defense Mobilization, and as chairman of the board of the National Science Foundation, he continued to promote government backing of scientific research for military and nonmilitary purposes.

But according to some sources — notably, fellow GAC member I. I. Rabi — Conant's interest in nuclear matters dissipated after the H-bomb battle, although he continued to attend and contribute to GAC meetings until his six-year term expired in September 1952. Early in 1951 the breakthrough ideas that would make thermonuclear weapons a reality were conceived by Teller and Stanislaw Ulam, and they were presented at a special conference hosted by Oppenheimer at Princeton University that June. Conant stayed away, for reasons that can be surmised from a letter to him from Oppenheimer: "From the first it seemed to me unlikely that you would come to the thermonuclear conference, or for that matter that you would much want to," Oppenheimer wrote. "There are some new thoughts which may be important for you to know; and I will tell you about them when I see you . . ." (166).

While dispelling doubts about the weapon's feasibility, the technical breakthroughs did nothing to soothe Conant's mounting alarm about what he regarded as a new and unhealthy relationship between science and the military. He was constrained from speaking out publicly on such issues as the hydrogen bomb, but he had aired his dissent in-



directly by criticizing what he termed shortcomings in the decision-making process for evaluating the merits of new weapons systems. In the January 1950 *Foreign Affairs*, and in a secret speech to the National War College the day after Truman's H-bomb decision, Conant urged the establishment of juridical-style review boards to resolve disputes involving science and government in research contracts, weapons development, etc. "The worst way to make decisions," he wrote, "is to resolve conflicts in favor of those with the loudest voice or the closest approach to political leaders" (167).

Two years later, in a secret address to military and government officials, he had grown even more worried; his remarks about the newfound technological impulse of the military resonate in today's debate over "Star Wars" and criticism of military equipment whose sophistication and advancement at times have overshadowed calculations of utility and efficiency. Before the atomic bomb, Conant recalled, a principal hindrance to weapons development was the "technological conservatism" of military officials who were "perhaps unduly slow in some cases to take up new ideas developed by the civilian scientists, full of enthusiasm who came down here with many wild ideas." But since then the situation had completely reversed:

It seems to me something like the old religious phenomenon of conversion. As I see it now, the military, if anything, have become vastly too much impressed with the abilities of research and development. They are no longer the conservatives. I don't know what I should say — at times they seem to be fanatics in their belief of what the scientists and the technologists can do. As I see it, in a word, the Defense Department is now like the story of the man who sprang on his horse and rode madly off in all directions; in other words, some of your colleagues have become infected with the virus that is so well known in academic circles, the virus of enthusiasm of the scientist and the inventor (168).

The GAC members who had lacked this enthusiasm about the hydrogen bomb and strategic bombing had fallen well out of step with prevailing currents in Washington, and by the spring of 1952 Oppenheimer's enemies in the Air Force, the scientific community, the FBI, and on the Atomic Energy Commission and the JCAE were mounting a quiet but concerted campaign to see that he, Conant, and DuBridge

were not reappointed to the GAC when their terms expired. Rumors of the impending purge reached Conant during a visit to Washington in May:

Lunched w V. Bush at Cosmos Club. Talk with Jim Fisk [former AEC aide, government science advisor], Oppie, Lee DuBridge. Some of the "boys" have their axes out for the three of us on the GAC of AEC. Claim we have "dragged our heels" on H Bomb. Dire words about Oppie!! (169)

By the time of his last GAC meeting on June 13–14, 1952, Conant was well aware that what one top Truman advisor privately termed a "clean sweep" of the group was imminent (170). At Conant's suggestion (171) the GAC had decided to prepare a report for president reviewing the AEC's progress since 1947, and his hand-written suggestions to Oppenheimer reflect awareness of the criticism received by the panel in the controversies of the prior several years, and deep uneasiness about the future. He defended his determined opposition to a rapid reactor program on the ground that proposed power-generating plants would not prove cost-effective compared to existing methods. He again warned that there was "grave doubt as to whether mechanisms exist for an adequate review of the pros and cons in regard to broad strategic questions involving the military and the AEC." He expressed disappointment at the paucity of advances in basic science, noting that the postwar atomic program was "living off the scientific ideas of [the] pre-1940 era." And, clearly thinking of the imminent testing of thermonuclear devices, he wrote, "The President should be aware of the lack of clear evidence as to the number of bombs that can be exploded without . . . endangering life. More accurate estimates must be obtained and should be in the mind of the President and his advisors" (172).

The final report — a somber recital of both the achievements and the dangers of the atomic effort — indirectly incorporated Conant's long-term fears (and his long-term hope for arms control) by noting the "temporary nature" of the U.S. atomic advantage. Therefore, "atomic armament, which is now held to be the shield of the free world, may in a foreseeable time become the gravest threat to our welfare and security." On the hydrogen bomb, the report made a point of observing



that it was "only for about a year, since the spring of 1951, that we have had promising and practical schemes for very large-scale thermonuclear weapons" (173).

As he walked from the War Department building on Twenty-First Street, Conant left behind him a momentous, sometimes exhilarating, sometimes painful, but finally grim stage of his life. His nuclear career was over, and his relief — even glee — was palpable: "Finally, Oppie, Lee DuBridge and I are through as members of the GAC!!" he scrawled in his diary. "10 1/2 years of almost continuous official connection with a bad business . . ." (174).

### Bitter Postscript: Conant and the Oppenheimer Hearings

Conant had left the nuclear business, apparently for good, but there was one bitter postscript to come. One of the most controversial episodes of that "bad business" recalled Conant to the netherworld of 1949 and the H-bomb dispute when he appeared before the Oppenheimer security hearing in April 1954. Conant, then serving as U.S. high commissioner to Germany, had learned from Bush of the move against Oppenheimer well before it became public. "In retrospect," he wrote Bush in late March, "I have no apologies whatsoever for the position which I took and to which all the other members subscribed in general. Indeed, I think I was as much a leader of this point of view as any person." To Conant, the fact that both the United States and the Soviet Union were rapidly progressing toward thermonuclear arsenals only solidified his belief that Truman's decision had been a disaster: "I think that the proposition which we put up to the Government, if it had been accepted, would have resulted in a better situation today than now exists." Masking his reference to the Oppenheimer case, then still under wraps, Conant noted that he had

even heard rumors that the patriotism of some or all of us involved in this recommendation has been impugned. The best answer to that, apart from the record of all of us, is the fact that the same group recommended such vigorous action in regard to the use of atomic weapons by ground forces as to bring about a revolution . . . (175).

The strongest evidence of Conant's anger at the proceedings against

Oppenheimer is his determination to testify despite strong discouragement from his superior, Secretary of State John Foster Dulles. Although Conant blandly relates in his memoirs that Dulles and President Eisenhower approved his wish to appear on Oppenheimer's behalf (176), surviving documents tell a more interesting story. On April 1, after receiving an appeal to testify from Oppenheimer's attorney, Lloyd Garrison, Conant made plans to return to Washington and sought to clear his appearance with Dulles. "If you do not think it improper to give what evidence I can in support of Mr. Oppenheimer's case, I shall proceed to do so," he wrote Dulles in a "personal and confidential" letter.

I have not the slightest doubt myself of his loyalty to the United States and his conscientious work for the United States Government in connection with all aspects of the atomic bomb development. I suppose that it is not incompatible with my position as United States High Commissioner for Germany to make my views known to this Personnel Security Board which is considering Mr. Oppenheimer's case (177).

Dulles's response was frosty. He first dispatched an "eyes only" cable to Conant claiming that "factors unknown to you make [an appearance] undesirable," then elaborated on April 12 in an "eyes only" letter from London:

Before I left Washington I checked up at the White House with reference to the problem mentioned in your personal, confidential letter to me of April 1, 1954.

I have the impression there is more on the adverse side than is generally known, and there is the general feeling in White House circles that it would be a good deal better if you did not become publicly involved in the matter. I do not mean to indicate there is any evidence to throw doubt on the gentleman's loyalty, and I do not think that any effort will indeed be made to prove disloyalty, at least as far as the Executive is concerned — I cannot vouch for what might happen in Congress. However, there is considerable evidence of laxity and poor judgment and, in some cases, lack of veracity.

I thought you ought to know this (178).

The issue came to a head when Conant, who had indicated to Bush and Garrison that he intended to testify, flew to Washington and met with Dulles on Monday, April 19 — the day before his scheduled



appearance. Only a brief note in Conant's diary survives to describe what was undoubtedly a tense encounter:

[...] [Saw the] Sec. for a brief 1/2 hr. Covered Germany in 15 min. Told him I had no choice but to testify at Oppenheimer hearings. He said I should know this might destroy my usefulness to govt. I said I quite realized this and he only had to give the word and I was through! (179).

Dulles, his bluff called, did not give Conant his walking papers, though the incident chilled their already-strained relations.

The next day, April 20, after an hour-and-a-half session with Garrison, Conant appeared before the board. His testimony is a model of restrained anger, clipped sentences, and a refusal to play along with the tactics of the AEC's aggressive attorney, Roger Robb. On the other hand it is frustratingly sparse, and constrained by the overweening propriety that was Conant's creed and contrasted with the refreshingly frank barbs of, for instance, I. I. Rabi. Conant did not pull any punches in the thrust of his testimony, however, steadfastly defending Oppenheimer and declaring that if, as the AEC's charge seemed to imply, a man's opinion as an advisor could render him unfit for government service, then "it would apply to me because I opposed [the H-bomb] strongly, as strongly as anybody else on the committee" (180).

After testifying, Conant took his concerns directly to Eisenhower, whom he had known well as a fellow Ivy League president when "Ike" headed Columbia. "Saw the President for 30 minutes," he wrote in his diary on April 26. "He opened up at once on the Oppie case. Prayed it would come out O.K., but doubted it." Conant told Eisenhower he was "very worried" about the H-bomb's inclusion in the charges against the physicist. He also related his showdown with Dulles, and received Eisenhower's assurance that "of course" it had been all right to testify (181). Troubled by his friend's appeal, Eisenhower later that day drafted a letter to "Jim" assuring him that

[...] no criticism was directed toward the Doctor because of his adverse opinion regarding production. That opinion was recited merely to give background to certain other allegations to the effect that, even after decision to produce had been made by the highest possible authority, the Doctor departed from his proper role as principal adviser and attempted to slow down development (182).

Eisenhower elected not to send the note, which is just as well, for Conant would not have found the argument persuasive: he wrote White House aide Bobby Cutler that only a clear-cut public statement could remove the widespread impression that Oppenheimer was being punished for his opinions (183).

When Conant heard the verdict of the hearing upon returning to the high commissioner's residence in Bonn, he privately vented his anger about this "first-class mess" that had been "badly handled in the White House": "It was a great error to have ever introduced the H-bomb into the indictment," he wrote Baltimore lawyer William L. Marbury; there

never would have been an Oppenheimer case if the scientists in the other camp had not circulated the rumours which they did. The basic trouble started as a row between technical advisers to the government so to speak and became very bitter. The administration should have known this and been guided by this fact. . . .

That Oppie was "vulnerable," i.e. had some things in his record which not read well, many of us suspected. I still stick completely to my statement about his loyalty — his judgment and discretion are another matter, and almost all the negative evidence refers to period a long time ago . . . (184).

Nine years later, when Oppenheimer was awarded the Fermi Prize by President Johnson, the sixty-nine-year-old Conant sent him a handwritten note: "Heaven knows you have earned it as a scientist many times over and much more . . . no prizes from the U.S. govt can wipe out the disgrace to the nation of your trial and judgment . . ." (185).

Conant himself lived long enough to be pleasantly surprised that mankind had not yet destroyed itself by employing all the devices so intermingled with his life. But until his death in 1978, he remained an inveterate "unenthusiast" about the hydrogen bomb and nuclear power, neither regretting his own atomic career nor confident that the arms control talks that had finally come to pass would dispel the nuclear predicament he had participated in creating. And despite a 1970 interview in the *New York Times* in which he seemed to concede that his position on the H-bomb had been in error — an admission that prompted a gleeful letter of inquiry from Lewis Strauss (with a copy to Roger Robb) to confirm that he had not been misquoted — Conant in his final, private pronouncement on the subject was more comfortable with the stand he had taken (186).



"I'm not completely sure that we were wrong, as most people would now say," he told an interviewer in 1974;

I wasn't too happy it was built. It couldn't win any war; it could only destroy the world. . . . I think that before the H-bomb was made there was a chance of controlling this thing, but the military wouldn't consider it. I think it was the wrong decision in that it couldn't possibly accomplish what we set out to do (187).

## Notes

1. Warner R. Schilling, "The H-Bomb Decision: How to Decide without Actually Choosing," *Political Science Quarterly* 76 (March 1961), 24–46.
2. Some important accounts of the H-bomb decision include Herbert York, *The Advisors: Oppenheimer, Teller, and the Superbomb* (San Francisco: Freeman, 1976); Richard G. Hewlett and Francis Duncan, *A History of the United States Atomic Energy Commission*, vol. 2, *Atomic Shield, 1947/1952* (University Park, Pa.: Pennsylvania State University Press, 1969), chap. 12; David Alan Rosenberg, "American Atomic Strategy and the Hydrogen Bomb Decision," *Journal of American History* 66 (June 1979), 62–87; Hans Bethe, "Comments on the History of the H-Bomb," *Los Alamos Science* 3 (Fall 1982), 43–53; McGeorge Bundy, "The Missed Chance to Stop the H-Bomb," *New York Review of Books*, May 13, 1982, pp. 13–22; Barton J. Bernstein, "Truman and the H-Bomb," *Bulletin of the Atomic Scientists* 40 (March 1984), pp. 12–18; and R. Gordon Arneson, "The H-Bomb Decision," *Foreign Service Journal*, May 1969, pp. 27–29, and June 1969, pp. 24–27, 43.
3. Hewlett and Duncan, *Atomic Shield* (2), covers the range of GAC activities.
4. *Ibid.*, p. 362.
5. For accounts of the Conant-Bush relationship and their wartime work see Richard G. Hewlett and Oscar G. Anderson, *The New World: A History of the United States Atomic Energy Commission, Vol. 1, 1939/1946* (University Park, Pa.: Pennsylvania State University Press, 1962); Martin J. Sherwin, *A World Destroyed: The Atomic Bomb and the Grand Alliance* (New York: Vintage, 1977).
6. Interview with Hans Bethe, January 1985.
7. James B. Conant, "Notes on the 'Trinity' Test," July 17, 1945, Bush-Conant Papers, folder 38, Office of Scientific Research and Development (OSRD) collection, S-1 files, National Archives, Washington, D.C.; the document appears in James G. Hershberg, "Ends vs. Means: James B. Conant and American Atomic Policy, 1939–1947" (undergraduate thesis, Harvard University, 1982), pp. 191–194.
8. Conant and Bush to Stimson, September 30, 1944, Harrison-Bundy Papers, folder 77, Manhattan Engineering District, National Archives, Washington, D.C.
9. Interview with Hans Bethe, January 1985.

10. Conant to Bush, "Possibilities of a Super Bomb," October 20, 1944, Bush-Conant Papers (7), folder 3.
11. *Ibid.* Conant first learned of the possibility of constructing an atomic bomb during a visit to England in March 1941; James B. Conant, *My Several Lives: Memoirs of a Social Inventor* (New York: Harper and Row, 1970), p. 277.
12. Interim Committee minutes, May 31, 1945, quoted in Sherwin, *op. cit.*, 1977 (5), pp. 297–298; Robert C. Williams and Philip L. Cantelon (eds.), *The American Atom: A Documentary History of Nuclear Policies from the Discovery of Fission to the Present, 1939–1984* (Philadelphia: University of Pennsylvania Press, 1984), p. 61.
13. Conant, *op. cit.*, 1970 (11), p. 278.
14. *Ibid.*, pp. 277–279.
15. Conant to Bush, "Possibilities of a Super Bomb" (10).
16. Conant to Bush, May 9, 1945, Bush-Conant papers (7) folder 38. Italics in original.
17. U.S. Atomic Energy Commission, *In the Matter of J. Robert Oppenheimer* (Cambridge, Mass.: MIT Press, 1971), pp. 228, 234. (Hereafter, IMJRO.)
18. See Bethe, *op. cit.*, 1982 (2), pp. 43–53; York, *op. cit.*, 1976 (2), pp. 20–28, 106–107. Bethe's article, originally written in 1954, echoed his testimony before the Oppenheimer board: "When President Truman decided to go ahead with the hydrogen bomb in January 1950, there was really no clear technical program that could be followed. This became even more evident later on when new calculations were made at Los Alamos, and when these new calculations showed that the basis for technical optimism which had existed in the fall of 1949 was very shaky, indeed. The plan which then existed for the making of a hydrogen bomb turned out to be less and less promising as time went on" (IMJRO, p. 33).
19. York, *op. cit.*, 1976 (2), p. 25; Hewlett and Anderson, *op. cit.*, 1962 (5), p. 32.
20. IMJRO, p. 236.
21. For instance, Bernstein, *op. cit.*, 1984 (2), p. 13.
22. Documents on the so-called "Fishing Party" may be found in the John Dulles Papers at Princeton University. An article on the committee's work is in preparation by the author.
23. Interview with E. F. Black, February 1985.
24. "Capabilities of the Weapons Mentioned in the Directive from the Secretary of Defense," undated [but apparently April 1949], National Archives, Record Group 330, CD-1-31 TSRD entry 199A.
25. Draft Fishing Committee, Final Report, circulated July 22, 1949, Fishing Party file, John Foster Dulles Papers, Princeton University.
26. Conant Committee, Final Report, October 15, 1949, National Archives, Record Group 330, CD-1-31 TSRD entry 199A.
27. James B. Conant, "The Impending Atomic Age: 1948 Preview," secret speech to National War College, September 14, 1948, copy in Conant Presidential Papers, Pusey Library, Harvard University.
28. *Ibid.*
29. Interview with Theodore Conant, January 1985.
30. James B. Conant, "Some Thoughts on the International Control of Atomic Energy," May 4, 1944, Bush-Conant Papers (7), folder 97.



31. Quoted by Keyes DeWitt Metcalf, forthcoming posthumous memoirs, ed. Edwin F. Williams; see James G. Hershberg, "Preserving 250 Million Pages of Knowledge," *Washington Post*, August 31, 1986, pp. A16–17.
32. James B. Conant, *Education in a Divided World* (Cambridge: Harvard University Press, 1948), p. 14.
33. James G. Hershberg, "James B. Conant and the Atomic Bomb," *Journal of Strategic Studies* 8 (March 1985), 78–92; Conant to Henry L. Stimson, January 22, 1947, Stimson Papers, Box 154, folder 18, Yale University Library, New Haven, Conn.
34. Quoted in Frederick Osborn, "Memorandum of Conversation," March 10, 1949, in Department of State, *Foreign Relations of the United States, Vol. 1, 1949*, pp. 39–43.
35. Interview with I. I. Rabi, January 1982.
36. In a March 1947 letter for AEC files attesting to Oppenheimer's loyalty after questions were raised about the physicist's left-wing associations, Conant stated that from 1941 "until the present day I have seen him intimately and discussed with him all manner of questions. During the war I visited Los Alamos frequently and in so doing came to know him very well. Since the war, I have discussed not only atomic energy for industrial and military purposes, but all phases of the international problem of control. Likewise, our conversation has ranged over the whole field of American politics and foreign policy. Therefore, I feel sure that the statements I make about him are based on an intimate knowledge of the man, his views, and his emotional reactions" (IMJRO [17], p. 378).
37. Interview with John H. Manley, November 1986.
38. Hewlett and Duncan, *op. cit.*, 1969 (2), p. 378.
39. Interview with John H. Manley, November 1986.
40. Hewlett and Anderson, *op. cit.*, 1962 (5).
41. Interview with Hans Bethe, January 1985.
42. IMJRO (17), p. 378.
43. Oppenheimer, A. H. Compton, E. O. Lawrence, and E. Fermi, "Recommendations on the Immediate Use of Nuclear Weapons," June 16, 1945, reprinted in Sherwin, *op. cit.*, 1977 (5), pp. 304–305.
44. Quoted in Alice Kimball Smith, *A Peril and a Hope: The Scientists' Movement in America, 1945–1947* (Cambridge, Mass.: MIT Press, abridged edition, 1970), p. 166.
45. Conant, *op. cit.*, 1970 (11), pp. 491–492.
46. IMJRO (17), p. 390.
47. Testimony of Hans Bethe, *ibid.*, p. 327.
48. James B. Conant, "The Atomic Age: A Preview, 1947 Edition," secret speech to the National War College, October 2, 1947, copy in Dwight D. Eisenhower Pre-Presidential Papers, 16–52 file, Box 27, Conant file (1), Dwight D. Eisenhower Library, Abilene, Kan.
49. Oppenheimer to Conant, October 29, 1947, Case file, Box 27, Conant file, J. Robert Oppenheimer Papers, Library of Congress, Washington, D.C.
50. Conant to Oppenheimer, November 2, 1947, Case file, Box 27, Conant file, Oppenheimer Papers (49).

51. Frederick Osborn, "United Nations Atomic Energy Commission Diary," February 18, 1948, entry, Osborn Papers, Harry S. Truman Library, Independence, Mo.
52. Conant, "Impending Atomic Age: 1948 Preview" (27).
53. Interview with John H. Manley, November 1986.
54. Conant, *op. cit.*, 1970 (11), pp. 493–494. A possibly decisive factor in Conant's decision to turn down the post was Truman's commitment to appoint Lewis Strauss, who later played a key role in ensuring that Oppenheimer lost his security clearance; interview with Theodore Conant, January 1982.
55. David E. Lilienthal, *Journals*, vol. 2, *The Atomic Energy Years, 1945–1950* (New York: Harper & Row, 1964), p. 354.
56. Hewlett and Duncan, *op. cit.*, 1969 (2), p. 337.
57. *Ibid.*, pp. 337–338.
58. Conant, "Atomic Age: A Preview, 1947 Edition" (48).
59. Conant, "Impending Atomic Age: 1948 Preview" (27).
60. Conant to Oppenheimer, March 7, 1949, Case file, Box 27, Conant file, Oppenheimer Papers (49).
61. IMJRO (17), p. 805.
62. Robert Jungk, *Brighter Than a Thousand Suns: A Personal History of the Atomic Scientists* (New York: Harcourt, Brace and Company, 1958), pp. 328–329.
63. Hewlett and Duncan, *op. cit.*, 1969 (2), pp. 363–366.
64. Conant social notebooks, Conant Personal Papers, Pusey Library, Harvard University. The notebooks also show that Conant and Oppenheimer enjoyed a Sunday dinner at Conant's on November 20, 1949, but only the menu (roast lamb, apple pie, ice cream) is recorded.
65. IMJRO (17), pp. 401–402.
66. Interview with Kenneth S. Pitzer, February 1985; Pitzer testimony, IMJRO (17), p. 699. However, Oppenheimer's desk calendar indicates that Pitzer visited a week later.
67. For a description of the "infinite containment" school into which the pro-H-bomb scientists have been placed, see Robert Gilpin, *American Scientists and Nuclear Weapons Policy* (Princeton: Princeton University Press, 1962), pp. 102–107.
68. Oppenheimer to Conant, October 21, 1949, reprinted in IMJRO (17), pp. 242–243.
69. Testimony of Edward Teller, IMJRO, p. 715.
70. IMJRO, p. 328.
71. Interview with Hans Bethe, January 1985. Though not asked about the letter during the hearings, Oppenheimer in 1957 privately recalled receiving "some kind of communication from Conant" in which he used the "over my dead body" phrase. Oppenheimer told a researcher, however, that "they were never able to find the letter." Warner R. Schilling, "Interview with J. Robert Oppenheimer, 11 June 1957 (12 June)," p. 6, Case file, Box 65, Schilling file, Oppenheimer papers (49). What happened to the letter remains a mystery. I was unable to locate it despite searching AEC, Oppenheimer and Conant manuscript collections. It would not be surprising if Conant asked Oppenheimer to destroy the letter at the time.



72. *Ibid.*
73. Alvarez diary, October 24, 1949, entry, IMJRO (17), p. 782.
74. IMJRO, pp. 243, 231.
75. Oppenheimer to Conant, October 21, 1949, IMJRO, pp. 242–243.
76. Lilienthal, *op. cit.*, 1964 (55), p. 577.
77. IMJRO, pp. 385, 387.
78. David E. Lilienthal diary, entry of October 29, 1949, Princeton University Library, Princeton, N.J.; Lilienthal, *op. cit.*, 1964 (55), pp. 580–581.
- 78a. Warner R. Schilling, "Interview with J. Robert Oppenheimer, 11 June 1957 (12 June)," pp. 6–7, Case file, Box 65, Schilling file, Oppenheimer papers (49).
79. Interview with George Kistiakowsky, January 1982; interview with E. Bright Wilson, Jr., December 1986; interview with I. I. Rabi, February 1982.
80. Interview with I. I. Rabi, February 1982.
81. James B. Conant, "Statement . . . (and Comments)" to the House Military Affairs Committee, November 29, 1945, Hearing on H. R. 4280, U.S. House of Representatives (Washington, D.C., 1945), pp. 51–59.
82. Conant to W. Barton Leach, April 26, 1946, Leach Papers, Box 52, folder 6, Harvard Law School Library.
83. Lilienthal, *op. cit.*, 1964 (55), p. 581. Manley believes that Conant may also have been referring to previous proposals he considered outlandish, such as the nuclear-powered airplane.
84. Borden interview with Federal Bureau of Investigation agents, February 20, 1954, FBI J. Robert Oppenheimer Serial file (100–17828), released under Freedom of Information Act. (Hereafter, JRO FBI.) This and other FBI Oppenheimer documents cited here were made available by Professor Martin Sherwin.
85. Conant, *op. cit.*, 1970 (11), pp. 49–50.
86. The GAC report is reprinted in York, *op. cit.*, 1976 (2), pp. 150–159.
87. *Ibid.*, p. 49.
88. *Ibid.*, pp. 154–155, 157.
89. Conant to Reinhold Niebuhr, March 6, 1946, Box 3, Conant file, Niebuhr Papers, Library of Congress, Washington, D.C.
90. Bernstein, *op. cit.*, 1984 (2), p. 13.
91. Conant, *op. cit.*, 1970 (11), p. 303; interview with Theodore Conant, January 1982.
92. James B. Conant, "Fight for Liberty," valedictory to Harvard undergraduates, January 10, 1943, in *Vital Speeches of the Day* 9 (February 15, 1943), 282.
93. James B. Conant, "Force and Freedom," *Atlantic Monthly*, January 1949, pp. 19–22.
94. For Conant's citation of international control as a factor in his support for using the atomic bomb, see Conant to Grenville Clark, August 15, 1945, Conant correspondence file, Grenville Clark Papers, Dartmouth University Library, Hanover, N.H.; James B. Conant, "Atomic Energy," *Texas Reports on Biology and Medicine* 5 (1947), 191; Conant to Harvey H. Bundy, September 23, 1946, Conant Presidential Papers (27); Hershberg, *op. cit.*, 1985 (33), pp. 83–84.
95. All quotes from the GAC report are in York, *op. cit.*, 1976 (2), pp. 151–159.
96. *Ibid.*, p. 156; Lilienthal, *op. cit.*, 1964 (55), p. 581.
97. York, *op. cit.*, 1976 (2), p. 156.

98. *Ibid.*, pp. 158–159; Lilienthal, *op. cit.*, 1964 (55), p. 582.
99. Interview with Lee A. DuBridge, January 1982.
100. Interview with I. I. Rabi, February 1982.
101. Hewlett and Duncan, *op. cit.*, 1969 (2), p. 385.
102. Lewis Strauss, *Men and Decisions* (New York: Doubleday, 1962), pp. 216–217.
103. Joint Chiefs of Staff, "Comments on Military View of Members of General Advisory Committee," January 13, 1950, reprinted in Thomas H. Etzold and John Lewis Gaddis (eds.), *Containment: Documents on American Policy and Strategy, 1945–1950* (New York: Columbia University Press, 1978), pp. 368–373.
104. Interview with Glenn Seaborg, February 1985; Hewlett and Duncan, *op. cit.*, 1969 (2), pp. 395–396.
105. *Ibid.*, pp. 394, 398.
106. The lunch is noted in a chronology of Acheson meetings on the H-bomb in the Acheson Papers, Truman Library, Independence, Mo.
107. Arneson, *op. cit.* (2), May 1969, p. 29.
108. Memorandum of telephone conversation with Admiral Souers by the Secretary of State, January 19, 1950, in U.S. Department of State, *op. cit.*, 1949 (34), pp. 511–512.
109. Lilienthal, *op. cit.*, 1964 (55), p. 581.
110. "Statement by the President on the Hydrogen Bomb," January 31, 1950, in Williams and Cantelon, *op. cit.*, 1984 (12), pp. 131–132.
111. Lilienthal, *op. cit.*, 1964 (55), p. 633.
112. Conant to William L. Marbury, June 30, 1954, courtesy of Mr. Marbury.
113. *Ibid.*
114. Conant to Oppenheimer, February 14, 1950, with Nat S. Finney to Conant, February 14, 1950, Case file, Box 27, Conant file, Oppenheimer Papers (49).
115. Conant to Bernard Baruch, February 24, 1950, Baruch Papers, Princeton University Library.
116. Portions of NSC-68 quoted in Etzold and Gaddis, *op. cit.*, 1978 (103), pp. 435–442.
117. Citations in this and the following paragraphs are from "Records of the Meeting of the State-Defense Policy Review Group, Department of State, Thursday, March 2, 1950," in Department of State, *op. cit.*, 1949 (34), pp. 176–182.
- ✓ 118. D. S. Greenberg, "The National Academy of Sciences: Profile of an Institution (II)," *Science* 156 (April 21, 1967), 360–364.
119. *Ibid.*, p. 360.
120. Joel H. Hildebrand, "'Vendetta?'" letter to *Science*, June 5, 1967, pp. 1177–78.
121. "Minutes of the Business Session, April 25, 1950," mailed on May 15, 1950; a second version of the minutes that omitted the names of Latimer and LaMer was mailed later at the suggestion of a member "anxious to suppress the names of the chemists [because] it will not look well . . . to emphasize that the coup d'état was engineered by chemists" (Edwin B. Wilson to Zwemer, June 3, 1950); both in National Academy of Sciences Archives, Washington, D.C.
122. Conant, *op. cit.*, 1970 (11), p. 498; Greenberg, *op. cit.*, 1967 (118), p. 361.
123. Greenberg, *op. cit.*, 1967 (118), p. 361.
124. Interview with George Kistiakowsky, January 1982.



123. Greenberg, *op. cit.*, 1967 (118), p. 361.
124. Interview with George Kistiakowsky, January 1982.
125. Wilson to Zwemer, June 3, 1950 (121).
126. "Minutes of the Business Session, April 25, 1950," May 15, 1950 (121).
127. Conant, *op. cit.*, 1970 (11), pp. 498–494; interview with William L. Marbury, August 1986.
128. Conant, *op. cit.*, 1970 (11), p. 499.
129. Interview with George Kistiakowsky, January 1982.
130. Kistiakowsky to Mrs. James B. Conant, May 1, 1980, courtesy of Elaine Kistiakowsky.
131. Kistiakowsky to Mrs. Conant, February 19, 1978, courtesy of Elaine Kistiakowsky.
132. Kistiakowsky to Mrs. Conant, May 1, 1980, courtesy of Elaine Kistiakowsky.
133. Greenberg, *op. cit.*, 1967 (118), p. 364.
134. William T. Golden, memorandum for the record: Interview with Lawrence R. Hafstad, January 4, 1951, courtesy of Mr. Golden; *The National Academy of Sciences: The First Hundred Years (1863–1963)* (Washington, D.C.: The Academy, 1978), p. 516.
135. Wilson to Zwemer, June 3, 1950 (121). Italics in original.
136. Conant, *op. cit.*, 1970 (11), p. 499.
137. Greenberg, *op. cit.*, 1967 (118), p. 361. He did please the academy, however, with his tactful handling of the affair in his memoirs; in 1975 the NAS council, in changing Conant's membership status to emeritus, voted to convey the group's "great admiration and respect for the spirit in which Mr. Conant accepted and described in his autobiography the sequents of events preceding the election of Detlev W. Bronk as President of the Academy" (NAS Archives).
138. Conant, *op. cit.*, 1970 (11), p. 499.
139. Hildebrand, *op. cit.*, 1967 (120).
140. Conant, *op. cit.*, 1970 (11), p. 498.
141. Interview with Kenneth S. Pitzer, February 1985.
142. Interview with Luis Alvarez, February 1985.
143. Latimer's testimony is in IMJRO (17), pp. 656–671.
144. *Ibid.*, p. 660.
145. *Ibid.*, pp. 663, 665.
146. *Ibid.*, p. 387.
147. March 5, 1954, FBI interview with Latimer, in memorandum by C. A. Rolander, Jr., dated March 15, 1954, JRO FBI serial file (84), #100-17828-947, p. 10. In this interview, Latimer contended that "DuBridge had supported Oppenheimer on the thermonuclear because of being naive and Conant had gone along with what he thought was the majority side."
148. Lawrence R. Hafstad, quoted in William T. Golden, memorandum for the record (134).
149. Edwin B. Wilson to Detlev W. Bronk, May 1, 1950, NAS Archives.
150. March 5, 1954, FBI interview with Pitzer, in memorandum by C. A. Rolander, Jr., dated March 15, 1954 (147), p. 8.
151. *New York Times*, March 8, 1952.

152. April 4, 1952, FBI interview with Pitzer, in SAC, San Francisco, to the Director, FBI (J. Edgar Hoover), April 5, 1952, JRO FBI serial file (84), 100-17828-275; J. Edgar Hoover to Rear Admiral Sidney W. Souers, special consultant to the president, April 16, 1952, JRO FBI serial file (84), #100-17828-291.
153. Interview with Kenneth S. Pitzer, February 1985.
154. March 5 and 8, 1954, FBI interviews with Alvarez, in memorandum by C. A. Rolander, Jr., dated March 15, 1954 (147), p. 18.
155. Interviews with Kenneth S. Pitzer and Luis Alvarez, February 1985.
156. May 1952 FBI interview with Teller, in report by FBI Albuquerque bureau dated May 27, 1952, AQ 100-1224, JRO FBI serial file (84).
157. Hewlett and Duncan, *op. cit.*, 1969 (2), p. 440.
158. *Ibid.*
159. Report of AEC liaison officer Chester Heslep, April 1954, quoted in Peter Goodchild, *J. Robert Oppenheimer: Shatterer of Worlds* (Boston: Houghton Mifflin, 1981), pp. 252–253.
160. Interview with Kenneth S. Pitzer, February 1985.
161. James B. Conant to Vannevar Bush, "Complaints about S-1 Project at Chicago Reaching the President," July 31, 1943, Bush-Conant Papers (7), folder 13.
162. David E. Lilienthal diary, January 28, 1950, Princeton University Library. Lilienthal excised the reference to Urey's spreading "innuendo on GAC" from the published version of the diaries.
163. Hewlett and Anderson, *op. cit.*, 1962 (5), p. 134.
164. IMJRO (17), p. 659; Hewlett and Duncan, *op. cit.*, 1962 (2), p. 537.
165. May 6, 1952, FBI interview with Willard F. Libby, in report of Chicago bureau dated May 9, 1952, JRO FBI serial file (84).
166. Oppenheimer to Conant, June 8, 1951, Case file, Box 27, Conant file, Oppenheimer Papers (49).
167. James B. Conant, "Science and Politics in the Twentieth Century," *Foreign Affairs* 28 (January 1950), 201; James B. Conant, "The Problems of Evaluation of Scientific Research and Development for Military Planning," speech to the National War College, February 1, 1950, National Defense University Archives, Washington, D.C.
168. Conant, "Problems of Evaluation of Scientific Research and Development for Military Planning" (167).
169. Conant diary, May 9, 1952, Conant Personal Papers (64).
170. J. Edgar Hoover to Tolson, Ladd, Nichols, July 10, 195, quoting Truman aide Adm. Sidney W. Souers, JRO FBI serial file (84), #100-17828-324.
171. Hewlett and Duncan, *op. cit.*, 1969 (2), p. 518.
172. James B. Conant, "Possibilities for Report to the President," June 1952, Department of Energy Archives; Hewlett and Duncan note only Conant's desire that "the President should be made aware of the results of Project Gabriel on the number of nuclear weapons that could be detonated without causing a health hazard" (*op. cit.*, 1969 [2], p. 518).
173. GAC to the President, June 14, 1952, declassified with deletions, Department of Energy Archives.
174. Conant diary, June 14, 1952, Conant Personal Papers (64).



175. Conant to Bush, March 26, 1954, Box 27, folder 614, Vannevar Bush Papers, Library of Congress, Washington, D.C.
176. Conant, *op. cit.*, 1970 (11), p. 501.
177. Conant to Dulles, April 1, 1954, John Foster Dulles Papers, General Correspondence and Memoranda Series, Box 3, Strictly Confidential N-P (2), Dwight D. Eisenhower Library, Abilene, Kan.
178. Dulles to Conant, April 12, 1954, and Dulles to Conant, undated cable (April 1954), both in Dulles Papers, General Correspondence and Memoranda Series, Box 3, Strictly Confidential N-P (2).
179. Conant diary, April 19, 1954, Conant Personal Papers (64).
180. Conant's Oppenheimer hearings testimony is in IMJRO (17), pp. 383-394.
181. Conant diary, April 26, 1954, Conant Personal Papers (64). Conant lunched the next day with Roger Adams, the foreign secretary of the National Academy of Sciences, and noted in his diary that Adams "said Nat. Acad. members divided on Oppie! The Calif. gang of chemists said he was a security risk. . . ."
182. Eisenhower to Conant, unmailed draft, April 26, 1954, Eisenhower Presidential Papers (Ann Whitman file), Conant folder, Eisenhower Library, Abilene, Kan.
183. Conant to "Bobby" [apparently Robert Cutler], April 30, 1954, copy in Lewis Strauss Papers, Conant file, Herbert Hoover Presidential Library, West Branch, Iowa.
184. Conant to William L. Marbury, June 30, 1954, courtesy of Mr. Marbury.
185. Conant to Oppenheimer, December 7, 1963, Case file, Box 27, Conant file, Oppenheimer Papers (49).
186. *New York Times*, March 9, 1970; Strauss to Conant, March 10, 1970; Conant to Strauss, March 30, 1970, all in Strauss Papers (183). Characteristically, Strauss maintained a file for the record on Conant containing possible derogatory information. On October 16, 1952, he filed a note stating that Robert LeBaron, chairman of the military liaison committee, "showed me reports, dated in 1944 and 1945 signed by V. Bush and J. B. Conant, and both advocating release of all atomic energy information to other nations, including Russia." Strauss did not note that the OSRD leaders had recommended such actions as part of a general international control plan.
- ✓ 187. Conant interview in John C. Landers, "The Manhattan Project, as Seen by Dr. Conant, And a Commentary on the Unprecedented and What It Has Left Us," March 1974 (unpublished ts), copy in Conant Personal Papers (64).